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THE INFLUENCE OF MENTAL AND MUSCULAR WORK ON NUTRITIVE PROCESSES.

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(*Read February 4, 1910.*)

We often wonder at the marvels of modern surgery and the wonderful advances made in surgery during the past fifty years. At the same time we are often surprised by the fact that medicine as such has not made corresponding advances and that doctors are inclined now to give less medicine than ever before. In one particular, however, medicine has made wonderful strides, namely, in the so-called "preventive" medicine. In fact, it is a common newspaper joke that ere long we will be employing our doctors and paying them only during such a period of time as they keep us well and thus stimulate their efforts towards using preventive medicine to its fullest. Pasteur, Lord Lister and Koch are household names today and stand for wonderful series of painstaking researches which have developed the fact that many diseases formerly thought inevitable in the course of a lifetime can, by reasonable care, in a large number of cases be avoided.

In order to bring preventive medicine to the present successful stage, innumerable experiments, involving microscopy, chemistry and physiology, were necessary to demonstrate in just what manner these baneful organisms (for in many instances they have proved to be organisms) enter into our bodies and produce diseases. Modern hygiene is based in large part upon the bacteriological researches of these pioneers.

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It is my privilege to explain to you how it is hoped to develop another line which, while it has by no means the attractive outlook presented by bacteriology and preventive medicine in general, may yet prove to be of the greatest value to mankind. I refer to investigations in the nutrition of man. If by proper study we can find what foods are best adapted to different purposes, if in what quantities they should best be ingested, what preliminary treatment is most desirable, we will have solved a great many problems regarding the diseases of digestion and will have made a large contribution to a wider branch of preventive medicine.

While the compound microscope can be used for studying the tissues, it requires a very different type of apparatus for studying the changes that take place in the whole body and the apparatus which, in our investigations, compares in a way to the compound microscope of the bacteriologist has the formidable name of respiration calorimeter. The microscope can reveal to us clearly what happens after the tissue is dead; it cannot, except in a few instances, give us a true picture of the processes which take place in the living body. These processes are extremely complex but we do know that as a result of food ingested, we obtain from the body heat, muscular work and mental work, and that there are certain excretions. During youth there is also a noticeable growth, while after the growth has been established, there is also repair of waste tissue. It is in studying these particular functions that we find it necessary to resort to a special apparatus—the respiration calorimeter.

We eat a great variety of foodstuffs and it is necessary for the body to so break down and rearrange the materials in these foods that the body can make the best use of them. For example, ordinary sugar cannot be used directly by the body but must first be broken down into dextrose and this dextrose is probably in part converted into another compound which is distributed through the muscles and particularly in the liver—a substance very closely allied chemically to sugar and called glycogen.

Like the frontiersman building his log house, the standing tree is of no use to him, but after the tree has been felled and the log hewn into the proper shape, then and then only can he begin to construct his house. But it is only during youth that the body is par-

ticularly engaged in building new tissue and the chief object of these rearrangements of the materials of the food is to so deposit the material in the various parts of the body that it can be used as fuel and be properly burned or oxidized. While, then, the food is first acted upon by the various digestive juices to prepare it for use by the body and as a result of digestive processes, the original material of the food is transformed to substances more or less closely resembling the components of the body, it is true, however, that when the body has completely and finally utilized these products, in general they are broken down to relatively simple compounds.

When coal or wood are fed to the boiler in the power house, heat is liberated and from the heat can be obtained power. In burning, the coal or wood is converted in large part into two simple products, carbon dioxide and water vapor. In order to convert the fuel into carbon dioxide and water, a large amount of oxygen gas is consumed and this, as you know, is obtained from the atmosphere.

We have frequently been told that the body is a good deal like a machine and consequently we can subject the body to tests very much like those given to a machine. For example, we can consider the food like coal—a fuel to the boiler in the boiler room—and the respiratory gases like the fuel gases leaving the chimney, and the feces and the urine like the ashes. With the boiler it is relatively simple to get a sample of the coal and analyze it and to take a sample of the ash and see what unburned portion of the material is present. It is somewhat more complex to take a sample of the flue gases and see how much unburned material passes up the chimney, but it is infinitely more difficult to make an experiment on a man. While we can analyze food, urine and feces, when we come to the respiratory gases, we must have a special respiration apparatus for the purpose.

Briefly, it is an air-tight copper-walled box, through which a ventilating current of air is passed. The air leaving the chamber contains carbon dioxide and water vapor produced by the man and is deficient in oxygen which has been taken out of it by the subject; this air is passed through purifiers where the carbon dioxide and water are removed and then the air is returned to the chamber to

be breathed over again, but before entering the chamber, the deficiency in oxygen is made up by admitting pure oxygen out of a steel cylinder, such as is used by a physician at the bedside.

It is possible with an apparatus of this type, then, to determine how much carbon dioxide and water are produced in twenty-four hours and how much oxygen gas is absorbed. If, together with

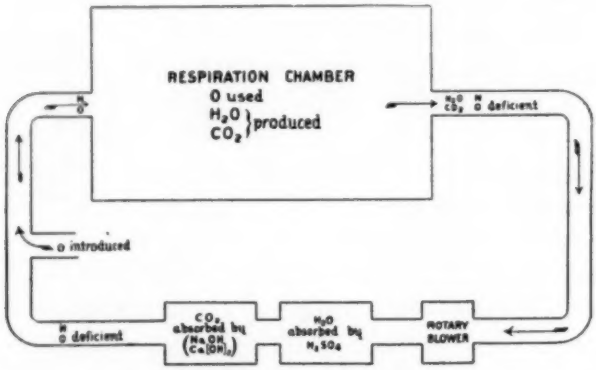
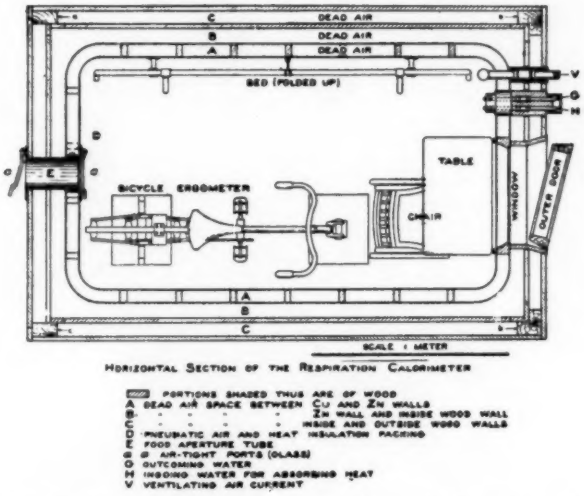


FIG. 1. Schematic outline of ventilation system in the respiration calorimeter at Wesleyan University, Middletown, Conn.

these data, we have the analyses of the urine and the feces, it gives us a very perfect picture of the transformations that have taken place inside the body during the period the subject has been inside the respiration chamber.

With the boiler, this is about as far as we can ordinarily go, as there is no satisfactory method for measuring the total amount of heat given off by the boiler in a large power house, but with a man it is possible to measure not only the products of combustion and the oxygen consumed but also to measure directly the amount of heat produced by the oxidation or combustion of the substances inside the body. This is the calorimetric feature of the apparatus. The air-tight copper box is surrounded by a number of cases which gives it a refrigerator type of construction and provision is made for the prevention of any loss of heat through the walls. There being no loss of heat, the man, acting as a small furnace, would soon produce enough heat to make him very uncomfortable. An ordinary man at rest gives off about as much heat as a 32 c.p.

electric lamp, 100 calories, so it is necessary for us to bring away the heat from the chamber as fast as it is liberated, or otherwise the man would become uncomfortably warm. As in the winter our houses are heated with hot water, this little house is cooled by cold water. Through a series of pipes inside the chamber is passed a current of cold water, then by noting the temperature at which the



HORIZONTAL SECTION OF THE RESPIRATION CALORIMETER

PORTIONS SHADED THUS ARE OF WOOD
A DEAD AIR SPACE BETWEEN Cu AND Zn WALLS
B " " " " Zn WALL AND INSIDE WOOD WALL
C " " " " INSIDE AND OUTSIDE WOOD WALLS
D PNEUMATIC AIR AND HEAT INSULATION PACKING
E FOOD APERTURE TUBE
d d AIR-TIGHT PORTS (GLASS)
O OUTCOMING WATER
H INCOMING WATER FOR ABSORBING HEAT
V VENTILATING AIR CURRENT

FIG. 2. Horizontal section of the respiration calorimeter showing heat insulation.

water enters and at which it leaves the chamber and the amount of water passing through, it is very easy to determine the exact amount of heat brought away in this manner. Thus this apparatus is well named a respiration calorimeter, "respiration" because it is an apparatus for measuring the respiratory processes and "calorimeter" because it measures the heat given off by the subject.

It is difficult for us to imagine that there may be heat or energy in a glass of ice cream soda or a piece of toast or a slice of ham and yet there have been one or two rather remarkable instances where some of our foodstuffs have been by force of dire necessity put to practical use for the production of heat. A few years ago the farmers of Kansas had an over-supply of corn and with the price of fuel as it was at that time, it became a common practice

among the Kansas farmers to actually burn corn. Several years ago, a large wheat steamer became stalled in lake Huron and in working her way against the ice for several days, exhausted her fuel supply. As a matter of fact, she was brought into Detroit by burning the wheat in her bins under the boilers. One of the most striking instances that I ever heard of was one that occurred on a coastwise steamer bound from Boston to one of the Maine ports. This steamer in the latter part of the year found herself encountering a severe gale and shortly the coal supply was wholly exhausted. Recourse was had to the demolition of the interior woodwork of the vessel, stateroom partitions, mattresses, furniture, but as a matter of fact, the steamer was brought into Portland harbor by burning a large cargo of hams under her boilers.

As we commonly eat our food materials, for the most part they are too moist to burn, although when the different food materials are deprived of a greater part of their moisture by drying, they burn quite readily. In so burning, they are converted to carbon dioxide and water and a definite amount of heat is liberated during this combustion.

Whenever organic material is burned and completely oxidized to carbon dioxide and water, there is a definite amount of heat liberated for each gram burned. Fats liberate very much more heat than do equal weights of sugars or starches, and protein, the third important element in our food, liberates about the same amount of heat as do the carbohydrates. If, therefore, we determine as we can with a special apparatus, the heating value or fuel value of different ingredients in our diet and then measure the total amount of food ingested, measure the heat produced by man and make due allowance for the heat in the feces and the solid matter of the urine, it is possible for us to strike a complete balance of income and outgo of energy and see whether our measurements are in error or not. So, on the one hand, we have a heat balance obtained by determining the intake and fuel and the output of unburned material in the ash and the heat directly produced. On the other hand, we have from the chemical analyses of the respired air and the excreta a means of knowing just how much protein, how much fat, and how much carbohydrates have been burned in the body during a cer-

tain experiment. If our chemical analyses have been accurate, we should be able to compute from the fuel values of protein, fat and carbohydrate thus obtained the heat these substances would be expected to produce when burned in the body. Obviously, this result should compare with the results as actually determined. When our apparatus functionates so perfectly that we can secure results in this way, we can feel that we are on safe ground and our results give us a very perfect picture of what takes place in the body. Having found such an apparatus and method, it only remains for us to apply this apparatus in as many ways as time and expense will permit.

Among the numerous questions that can be solved by an apparatus of this type is the effect of work on changes in material in the body, which we call metabolism. We know that when a man is sitting quietly at rest in a chair, he gives off, say, 100 calories of heat per hour. This is about the amount of heat given off from a 32 c.p. electric lamp. If, however, a man stands up and walks about, we have found that his heat production rises considerably and we find, also, that not only does the heat elimination increase but the carbon dioxide in the breath increases correspondingly.

Muscular exercise with varying degrees of intensity produces varying amounts of carbon dioxide and heat as is shown by the table

TABLE I.

AVERAGE NORMAL OUTPUT OF CARBON DIOXID AND HEAT FROM THE BODY.

Conditions of Muscular Activity.	Average Quantities per Hour. Carbon Dioxid. Gms.	Heat. Cals
Man at rest, sleeping	25	65
Man at rest, awake, sitting up	35	100
Man at light muscular exercise	55	170
Man at moderately active muscular exercise	100	290
Man at severe muscular exercise	150	450
Man at very severe muscular exercise	210	600

herewith. The classification of muscular exercise on this basis is very unsatisfactory, as different people may have different impressions of what is meant by "moderately active muscular exercise," for example. It is certain, however, that under these conditions, the characterization so far as the sleeping period is

concerned is pretty well fixed and it is also true that man at very severe muscular exercise means practically the limit of human endurance.

Using the values in this table it is possible to roughly compute the average daily output of heat from a man at light muscular work or the heat from a man with different degrees of muscular work. Assuming, for example, that the man is at light muscular work, the computation is as follows as given in Table II herewith.

TABLE II.

AVERAGE DAILY OUTPUT OF HEAT OF A MAN AT LIGHT MUSCULAR WORK.

Daily Program.	Heat Output.
At rest, sleeping, 8 hours, 65 calories per hour.....	520
At rest, awake, sitting up, 6 hours, 100 cal. per hour.....	600
Light muscular exercise, 10 hours, 170 cal. per hour.....	1,700
Total output of heat, 24 hours	2,820

The computation of the average daily output of heat on this basis is at best somewhat unsatisfactory, as we do not know with sufficient accuracy the heat output during the various degrees of muscular activity. Nevertheless, it serves to give a general idea of the possibilities of using standard normal values for getting a rough approximation of the energy output of different men with different conditions of muscular activity.

We are all of us firmly of the opinion that when we are doing strenuous mental work, we are doing a great deal of work. We feel sometimes perfectly exhausted at the end of a severe mental task, and consequently it will be interesting to note to what degree mental work influences the interchange of material in the body. Does severe mental application call for a greater production of heat or a greater oxidation of the tissues? In order to test this question satisfactorily, it became necessary for us first to try to find out how to get men to work hard mentally for a period of several hours.

While at Wesleyan University, it occurred to me that it might be desirable for us to study the metabolism of students during the mid-year examination period, for if there is any time in which a college student performs mental work, it is during the examination period. Consequently it was arranged for students to take

their examinations in the respiration calorimeter.[1] The chamber is large, fairly well lighted, quiet, with a good ventilation and every possible convenience for quiet, sustained mental effort without distractions. The men entered into the experiments heartily and twenty-two such experiments were made. In order to control the experiments and give us a basis for comparison, the same men were on subsequent days placed in the calorimeter for the same length of time, during which period no mental work was done and, indeed, we went so far as to attempt to eliminate the results of the muscular work of writing by giving them plain copying to do on paper in the control period. By this means we were able to compare the results of the experiments with mental work with the control experiment. During these mental work experiments, the men were encouraged to note down all their personal impressions. Some of the observations were very interesting. Several men said it began to grow cold and then to grow warm, while as a matter of fact, the temperature of the chamber did not vary by more than two or three hundredths of a degree. Other men noted that they perspired freely during the examination period and that they had been under a tremendous strain and effort.

TABLE III.

METABOLISM DURING MENTAL WORK.

(Quantities per hour.)

	Examination Period.	Control Period.
Carbon dioxide.....	33.4 grams	32.8 grams
Oxygen	27.3 grams	25.9 grams
Water vapor.....	39.2 grams	37.8 grams
Heat	98.8 calories	98.4 calories

Averages of 44 experiments.

But all of these personal impressions fall wholly out of consideration when we compare the results of the experiments with the students during the examination period and during the control period. There was practically no difference between the results with

[1] For a detailed discussion of these experiments see Benedict and Carpenter, "Muscular and Mental Work and Efficiency of the Body as a Machine," United States Department of Agriculture, Office of Experiment Stations, Bul. 208, 1909.

the men during the mental tests and during the control periods. This, to the layman, is certainly a most surprising outcome, for, as you know, it is the popular impression that a sustained mental effort results in a complete physical exhaustion. Here, then, is a problem that is pretty thoroughly solved by means of this large apparatus which had been constructed for experiments of exactly this kind.

I ought to add here, perhaps, that incidentally among the old legends that have been handed down to us and still find credence in public opinion is the belief that fish is an excellent brain food. This has long been wholly exploded. In fact, it has been easily traced to an old saying of Professor Moleschott,— "Ohne Phosphor kein Gedanke," and as fish is known to contain phosphorus, it was thought that fish was an excellent brain food for mental workers. As a matter of fact, there are no researches that show that there is any influencing of the chemical processes of the body as a result of mental exertion.

Of special interest, perhaps, from the standpoint of hygiene in these experiments is the fact that several of the men expressed a feeling of restraint and a lack of freedom to stretch themselves and possibly to move about somewhat as a result of staying in the chamber. Of course our experiments would have been seriously inconvenienced if the men had moved about freely, so that we asked them to diminish as much as possible all extraneous muscular exertion other than that required in using the pen. This longing for the use of the arms and body during mental exercise is simply another means of indicating that our bodies need as perfect circulation in all parts as possible, that when long-continued and cramped positions are maintained, there is a feeling of uneasiness and discomfort.

It is a most wise and growing custom for book users to vary the body position during sustained mental work. The increasing use of the standing desk while reading, and the pacing of the floor during mental work or dictation of addresses and literary productions testify to the fact that for best mental effort, there should be physical exercise, though this is far from affirming that the center rushes invariably become literary men. In our studying we should certainly pay more attention to physical exercise and fresh air, and

the old picture of the bookworm with his feet on the top of the desk, the back hunched into an arm chair and the head enveloped in tobacco smoke can hardly be considered as portraying the ideal condition for creative work in the light of modern physiology and psychology.

Muscular Work.

While mental effort is without appreciable influence on metabolism, I now wish to call to your attention a most interesting series of experiments on the influence of severe muscular work, particularly such as bicycle riding, upon the transformations of the body. The narrow confines of our respiration chamber make it necessary for us to restrict our experiments to those forms of muscular activity that do not require a large amount of apparatus or large space. A form of stationary bicycle called a bicycle ergometer is ideal for such experiments. We placed inside the respiration chamber a stationary bicycle, the rear wheel of which was replaced by a large copper disk rotating in a magnetic field. With this apparatus it was possible to put an electric brake, so to speak, on the copper disk and make it more or less difficult to drive the pedals Without going into details, I will simply say that it is possible to calibrate this apparatus and to know that every revolution of the pedals with varying strengths of the electric brake results in the transformation of a definite amount of energy into heat, consequently when a man rides, we have only to count the number of revolutions of the pedals to know how much heat has been transferred from the muscles of his body to the pedals in the form of effective work. This apparatus, together with the respiration chamber, has given us a means of studying man as a machine in a way that I think has never been done before.

One of the important problems of interest to the engineer is the efficiency of his engine. How much energy of the coal can a power plant convert into effective mechanical work? Obviously, the best power plant is that which produces the best mechanical work out of one ton of coal. With man, we also have a machine. The fuel in the boiler is again the food and the muscles are the levers by which the energy is transferred from the muscles to some effective

apparatus so that it can be used as external work. This apparatus, the bicycle ergometer, is admirably adapted for the use of the powerful leg muscles and hence we were able to study this to a considerable degree.

The experiments were made something in this way. The subject was first placed in the respiration chamber for part of a day and the heat production measured during rest, while he was sitting quietly reading a book. This is our "base line," as the surveyor says, or our basis for comparison. On subsequent days similar

FIG. 3. Bicycle ergometer for studying problems relating to muscular work.

experiments are made in which the man rides the bicycle ergometer with varying resistance of the electric brake. During these experiments the heat production and the carbon dioxide eliminated are

measured directly and also the amount of work in terms of calories put on to the bicycle accurately determined by noticing the number of revolutions of the pedals. We thus have a determination of the total heat production and the heat of mechanical work which we can compare with the total heat production and thus find the efficiency. The degree of resistance of the electric brake varied from mere coasting when no resistance is applied but the legs were made to rotate as in coasting down a hill without a coaster brake, to such a high resistance that the wheel would hardly be carried by the dead point. The subjects used for these experiments varied. Two of them, coming out of the wild woods of Maine, had never seen a bicycle. Others were college athletes and were on the college track team, and finally one of the most interesting experiments was made with one of America's professional bicyclists, Mr. Nat Butler of Cambridge.

In Table IV, I have summarized a number of experiments with these different subjects.

TABLE IV.

EXPERIMENTS ON BICYCLE RIDERS.

(Calories per Hour.)

	Resting.	Working.[2]	Work Done.	Efficiency, Per Cent.
J. C. W.	112	339	49	21.6
B. F. D.	106	318	45	21.2
A. L. L.	105	326	46	20.8
E. F. S.	117	399	51	18.1
N. B.	92	619	112	21.3
		471	79	20.8
		401	65	21.0
		382	60	20.7

In the first column under the title "resting" is recorded the number of calories produced per hour by these different men when sitting quietly reading in the respiration calorimeter. These experiments are almost identical in their nature with the control experiments in the mental work experiments and are intended to show the heat requirement for maintenance during rest. These men gave off, as you see, not far from 100 calories per hour, which again is about the heat production of a 32 c.p. lamp. As they were

[2] Including heat produced by the ergometer.

all of athletic build, the heat production is somewhat larger than would be expected from men in sedentary occupations and you will remember in the mental tests, the heat production per hour averaged 99 calories. The average here is a little higher and is readily accounted for by the fact that the men were muscular. In the second column is recorded the heat production per hour during the period when the subjects were engaged in severe muscular work on the bicycle ergometer. Here the differences between the different subjects are very great. The lowest heat production was in the case of B.F.D. 318 calories, and the highest nearly twice as great, namely, 619 calories by N.B. While at first sight, it would appear that there was a very marked difference in the work or efficiency from these figures given in this column, it is necessary for us first to find out how much of this enormous heat production was actually converted into mechanical work, and we find that in the third column there were very large differences in the amounts of work accomplished by these men. These differences can be explained on two grounds, first, the rate of revolution of the pedals was different, and second, the degree of resistance in the electric brake was very different. It is interesting to note that the first three men during work gave off about 330 calories per hour and accomplished about 46 to 47 calories of effective work, thereby showing very little difference in either the total heat production or in the work done. The fourth man, E.F.S., gave off considerably more heat during the working period and also accomplished more work; and then we come to the astounding results of the experiment with Mr. Butler in which he gave off 619 calories and accomplished the enormous amount of 112 calories of muscular work. The remaining experiments with Mr. Butler were made with the idea of decreasing the muscular work and noting the effect on the heat production. You can see in a general way that when the total heat falls off, the work done falls off.

Perhaps of greatest interest is the comparison of these different men with regard to their efficiency. There are a number of methods whereby the efficiency can be computed from the figures given in the table. Theoretically we can say that in the first instance for every 49 calories of heat produced, there were required 339 calories

of total energy. This would give us an efficiency of not far from 14 per cent. On the other hand, another method of computing the efficiency, and perhaps one that is fairer, is the method in which the total resting metabolism is deducted from the total heat production and we compare the work done with the increased amount of heat required to do this work. Thus, in the first experiment out of 339 calories produced during work, 112 calories can reasonably be assigned to the production necessary for maintenance while the subject is sitting quietly reading in the calorimeter. Deducting the 112 from 339, we find that 227 calories of heat result in the production of 49 calories of efficient work. On this basis, the efficiency is 21.6 per cent. A similar calculation for the other subjects shows the remarkable fact that there are but slight differences between these men as regards their efficiency. In other words, for every calorie of efficient muscular work done, there are about 5 calories of extra heat produced. In the case of the enormous heat production of Mr. Butler, we find that the differences in the two methods of computation are not as noticeable for it makes but a small change in the percentage whether we divide 112 multiplied by 100 by 619 or whether we divide 112 times 100 by 527. In the latter case, the efficiency is 21.3 per cent., while in the first it is about 18 per cent. Obviously, the greater the amount of work accomplished per hour, the less the influence of deducting the resting metabolism.

How does a man compare with an engine as regards efficiency? I have no doubt but that some fault could be found with my reasoning when attempting to compare a man with a machine but the best steam engines of the present day do not average an efficiency much greater than 14 per cent. Certain internal combustion motors realize somewhat more. At any rate, we can say that a man is a wonderfully efficient machine.

The figures in Table IV shed a most interesting light on the question of training. It has commonly been supposed that when a person is trained, the muscles become more effective and consequently there is a greater production of work for the same expenditure. In these figures here we find that in the first place the two men, A.L.L. and E.F.S., who were wholly untrained, and indeed wholly unfamiliar with the bicycle, accomplished as much work as

did the college athletes, J.C.W. and B.F.D., and, indeed, with an efficiency very little less than that of the first two. When we come to the figures of Mr. Butler, we find that he was able by virtue of his skill and strength to accomplish a very great deal *more* work than any of the other men, but as a matter of fact, his *efficiency* was not materially greater than that of the college athletes, or indeed, the untrained men. So far as these two untrained men are concerned, their training consisted in riding the bicycle ergometer one half hour the first day and a half-hour increase for each succeeding day for six days prior to the test. Thus on the sixth day they rode three hours at one stretch and the total training occupied but 10.5 hours. This was done in an attempt to accustom the leg muscles to the exercise before the experiment began, so that they would not be sore when used in the subsequent experiment, so while they did have a small amount of training, it was far from the ordinary training a college athlete would pass through in preparing for an intercollegiate contest. It is obvious that Mr. Butler was able to accomplish an enormous amount of work by virtue of his long experience and well-developed musculature, but it is indeed astounding that his muscles had no greater efficiency than did those of much less trained college athletes.

It is very clear from these experiments that in order to produce muscular work, there must be a very large by-production of energy in the body. When we consider that a man at rest, sitting quietly, requires 92 calories for his maintenance, and when we know, as we do from other experiments, that the same man asleep would require not far from 60 calories, we see that in Mr. Butler's case during a severe muscular work period, there was a heat production in his body amounting actually to 10 times that when he was asleep.

In order to produce this heat in the body there must have been combustion, vigorous combustion, combustion either of body substance, in case the subject did not have food enough, or combustion of food material previously eaten. We have found as a result of a large number of experiments that a man at rest, doing no visible external muscular work, requires not far from 2,000 calories for maintenance during twenty-four hours. You can see that in three hours Mr. Butler produced nearly this amount when at severe mus-

cular work; consequently he must have burned up in these three hours as much material as would ordinarily be burned by a subject at rest in twenty-four hours. On this same basis, he would need three meals every three hours or one square meal an hour.

I think figures like these go to show most conclusively that the proposition we hear frequently made to cut down our total food consumption is founded on entirely erroneous grounds. If we do muscular work, we must burn up material. We must draw on body substance or we must supply food. I have no doubt that many of us are too fat, not grossly over-weight, but somewhat over. People of sedentary habits have a tendency to take on weight and become 25 or 30 pounds over-weight without much difficulty, but I contend that the average man is not too fat and that his diet is not too large for him. People say continually, "We eat too much." Doubtless at times we do eat too much. I have no doubt that every one of us remembers the feeling of satiety with which we rose from our last Christmas dinner. Doubtless this feeling of satiety was carried farther than health, and in some cases, good breeding would justify, but Christmas dinners are not an everyday occurrence and it is not logical to say that because we over-eat on one day, that we continually over-eat.

It is the custom of athletes in general to consume large amounts of proteid food, particularly beef, eggs and milk. There is a tradition which has been handed down for many years that this diet is best suited for athletes, and that a large quantity of protein is advisable for a high efficiency and large and sudden drafts on strength. It is beginning to be questioned whether or no this after all is the logical diet for training. I have not time to go into this discussion, but there are at present some very strong advocates of a low protein diet, both for ordinary life and for athletic training and while in my mind their case is far from proven, some of the arguments are certainly most striking in their effectiveness, showing that our good friends, the vegetarians, have developed a remarkable degree of endurance when compared with their meat-eating competitors. All of these problems will gradually be worked out and on a sound scientific basis, but they have nothing to do with the general

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fact that when muscular work is accomplished, food must be supplied, and we cannot cut down our diet without losing flesh and simultaneously cutting down our muscular work. A great many men who consider themselves as living a sedentary life, actually, accomplish much more muscular work than they realize. For example, I have made an experiment a good many times by carrying a pedometer. During one winter when I was writing a large report and my exercise was confined to walking to and from the laboratory, two blocks from my house, and going about among the workers in the laboratory, I thought I was doing a very small amount of work, having almost no exercise, and yet I found to my astonishment that I recorded almost unerringly every day a distance walked of about seven miles. Obviously during that winter, since I held my body weight, my food must have been instinctively so adjusted as to exactly supply the demands required for the winter's work. When we consider that the intake of food is determined in a large measure by appetite and the feeling of satiety and the consumption of the food is dependent on the muscular activity, I do not know any factor regarding the body functions that is any more delicately adjusted than is the balance between the intake of food and the food requirement.

I think, then, we can take it as thoroughly settled that for the large majority, if the appetite is ordinarily followed, it will result in a most perfect adjustment of the food intake to the food requirement. Obviously it is important that we select foods that agree with us; excessive amounts of sweets and foods difficult of digestion are certainly to be avoided, but whether we follow the no-breakfast or the no-dinner or the no-supper plan, it is absolutely certain that in the course of twenty-four hours, or perhaps in the course of one week, what we lost in the meal voluntarily given up is compensated by increased consumption at the other meals. This I know is contrary to personal impression but we must, if we wish to make accurate observations on our diet, wholly eliminate our personal impressions. Nothing but the scales and an accurate table of analyses of food materials will give us results of any value. My personal belief is that instead of giving up one or two meals a day, it would be better for us to eat more often and less

in amount. Particularly is this the case with those who are desiring to reduce the body weight. It is sometimes amusing to think of people who are going through all kinds of muscular exercise to work off their fat and at the same time these muscular exercises are stimulating a most voracious appetite which must be appeased. In my judgment, instead of decreasing the consumption of food to reduce the fat of the body, it is better to produce a feeling of satiety and thus cut down the ravenous appetite. This can, I believe, best be done by more frequent small meals than by leaving out any one meal and eating to complete satiety at the others.

I have outlined to you very hastily this evening a few of the important problems that we are able to study by means of the respiration calorimeter. These researches that I have thus far outlined were carried out in the chemical laboratory of Wesleyan University and it may be of interest to you to know that the researches were deemed so important that a special laboratory has been constructed for their prosecution in Boston—the Nutrition Laboratory of the Carnegie Institution of Washington—where it is hoped to study more accurately the questions relating to health and, indeed, to disease. The laboratory is located near the Harvard Medical School and in the vicinity of a large number of hospitals which are either built or are planned. In this way we hope to be able to get into most intimate touch with pathological material.

At Wesleyan University only one calorimeter was used for all the different kinds of experiments, but in the new laboratory three calorimeters are already completed and two others projected, thus providing an apparatus which will be particularly adapted to the kind of experiment planned. In the chair calorimeter a large number of experiments have already been made with diabetic subjects, and the bed calorimeter has been used for studying bed-ridden patients, not only for those having diabetes but a most interesting series of observations have also been made on women before and after confinement. The laboratory is also thoroughly equipped for studying all the excreta and many problems involving muscular work.

THE TUNNEL CONSTRUCTION OF THE HUDSON AND MANHATTAN RAILROAD COMPANY.

By JOHN VIPOND DAVIES.

(*Read February 18, 1910.*)

The tendency of population in the civilized world, as it increases with advancing years, is toward congregation into great cities instead of a uniform distribution, and this, as is readily understood, is due to the tremendous concentration of manufacturing industries and commerce and the interrelated business brought about and made possible by improvement in the world's transportation facilities.

While the tremendous growth of modern cities is due to transportation facilities, yet the very increase in population and area has created local transportation problems, within the cities, which were not dreamed of only a century ago, even in the great cities of the old world. This increase in the size of the old cities could not be foreseen; so that, instead of their growing in accordance with a preconceived plan, having ample provision for arteries of communication, we have, unfortunately, development without a plan, the newer portions spreading over the surrounding area, merging intervening villages and towns, and creating problems which can only be solved at great expense. Even in the cities of the newer countries, this growth could not have been foreseen or adequate provision made for it.

As an indication of how little this tendency of population to concentrate in the cities could be foreseen, we have only to consult the United States Census to find that at the first census, in 1790, three-tenths per cent. of the population were living in six cities of eight thousand population or over, while in 1900 thirty-three and one-tenth per cent., or about one third of the population, were living in five hundred and forty-five such cities, and that the population of greater New York is now equal to that of the whole of the United States in 1790. During this period the population of the United

States has increased over nineteen times, while that of the area now comprising New York City has increased seventy times.

New York is the heart and center of the second largest aggregation of people in the world, and differing from London, which ranks first, has reached its present position during a comparatively short life. New York was not laid out as a complete city, but has developed by the fusion of scores of villages. Manhattan Island, when it became populated north of Greenwich village, and was mapped on the present plan, was so laid out because at that time it was contemplated that the North and East Rivers, being the main arteries of traffic, needed the greatest number of thoroughfares with the closest intervals, to connect the water fronts. Consequently, the cross-town streets are spaced close together (about 260 foot centers) while the north and south avenues are widely separated. The growth of New York far to the North and East, beyond the East River, has entirely altered the traffic conditions and has produced one of the most difficult conditions in the solution of the present problem. The municipal limits of the Greater City of New York includes, by no means, all of the metropolitan district, which comprises all the suburbs tributary to New York as a place of business, all of which is essentially part of the aggregation of population to be dealt with in the traffic problem. The estimated population of New York City on December 31, 1909, was 4,516,000. The suburbs on Long Island and Westchester County represent 325,000, while the suburban district in New Jersey represents 1,691,000, making a total of 6,527,000 persons.

The last report of the Public Service Commission returns the total passenger transportation in Greater New York in 1908, on the various subway, elevated and surface lines, omitting transfers, as 1,358,000,000 rides. The service in the New Jersey district was 284,000,000 rides. In that year the steam railroads hauled within the same district over 100,000,000 persons, so that the total passenger rides per annum in the city and vicinity of New York amounts to the fabulous total of 1,742,000,000, an amount equal to one ride per annum for each person in the world.

In the Borough of Manhattan this traffic represents over 400 rides per head of population per annum, while in the entire city

the movement has grown, by steady progression, from 164 rides per head of population per annum in 1884, to 300 rides in 1908. The advance in this growth has been marked as each improvement in methods of transportation has been introduced, particularly when electric traction succeeded steam, horses and cable, and again when the subways were opened.

Prior to the opening of the various bridges and tunnels, the ferries in New York Harbor carried 208,000,000 persons per annum, of which amount some 120,000,000 crossed the Hudson River.

No other excuse or explanation than these figures is needed for the construction of the various tunnels under the North and East Rivers.

In the cities the main arteries are unduly congested, and many of them are inadequate to provide for the various classes of transportation imposed upon the surface. In some cases the capacity has been increased by construction of elevated railroads, which, while convenient for the passengers and cheap to construct, are a serious impediment to the full use of the surface, are eyesores to behold, and are a nuisance to the health and nerves of the general public. The use of the sub-surface then, is essential to the development of the increased facilities for rapid transit, where the railroad can be out of sight and its operation out of hearing; where it is not affected by climatic conditions, and where the maintenance costs of structure and equipment is minimized.

In our cities the development of subways near the surface, while convenient for public use, introduces very serious questions in relation to sanitation. The first subway built can be carried out by extensive diversion and reconstruction of the sewerage system, but a point must soon be reached where the entire sewerage system must be considered as preëmpting a definite horizontal section upon which no subways may encroach. After that, subways can be laid out with hardly any limit, at greater depths and to almost any nummer, tier upon tier, as necessity demands, in the solid rock foundation underlying our streets.

We are rapidly getting to this point in New York, as is illustrated at Sixth Avenue and Thirty-third Street, New York, where, in addition to the elevated and surface lines, provision is made for a

Broadway Subway near the surface, below that the Hudson and Manhattan Tunnel, below that again the Pennsylvania Tunnels, and for a cross-town line at a level below these.

The Hudson and Manhattan Tunnels are the direct lineal descendants of the original Hudson River Tunnels commenced in 1873. They are developed from the traffic conditions outlined above.

The original undertaking, if it could have been carried to completion at that time, would, unquestionably, have proved unsuccessful in operation. It was then contemplated to build this tunnel from Fifteenth Street, Jersey City, about midway between the Erie and Lackawanna Railroads to a union station at Washington Square, New York, and to transport therein the steam engines and trains of all the railroads terminating on the westerly shore of this river. The method of construction adopted by Colonel Haskins, while feasible for working in the river silt, would never have been adequate to complete the work, and it is only in the past decade that the art of engineering has reached the point of providing the methods for construction and of operation suitable to the completion of this work and to meet the demands of the travelling public, within the scope of modern operation.

In the subject matter following, describing the methods of tunnel construction, the work of the Hudson and Manhattan Railroad tunnels is directly considered, but except in size of structure, and as providing for totally different types of equipment and operation, the matter is in every respect applicable to the work on the Pennsylvania Railroad tunnels, and it may be noted that the ratios of tunnel sizes to weight of equipment are almost the same in the two propositions. The Hudson and Manhattan system comprises four complete tube tunnels under the Hudson River, a belt line connecting the principal steam railroads terminating in Jersey City and Hoboken, and two terminals in New York City—one up-town and one downtown. It is essentially a distributing and collecting terminal in New York for the transportation lines in New Jersey; and a distributing and collecting agent in New Jersey for the people of New York. Its building has involved more varied character of construction than any underground project ever executed, and cer-

tainly represents every known type of tunnel construction. The most spectacular portion of the work, but by no means necessarily the most difficult, is the tunnelling under the rivers.

The Hudson Valley is a deep and narrow gorge, having on the west bank the red sandstone formation of the Newark series, and on the east side the micaceous gneiss of New York. For the most part, this valley—the floor of which is some 250 feet below sea level—is filled with silt. This material at the depth of the tunnel sections is a firm substantial clay. Its specific gravity, wet, is about 1.65, dry, about 2.50; weight per cubic yard, wet, about 103 pounds, dry about 156 pounds. Its chemical analysis is as follows:

	Per Cent.
Water	29.50
Silica	47.52
Iron oxide	2.69
Alumina	11.66
Lime	1.88
Magnesia	1.18
Soda	1.80
Potash	1.82
Chlorine	0.30
Sulphuric anhydride	0.11
Carbon dioxide	0.88
Phosphoric acid	0.13
Organic matter	0.53
	100.00

It will flow under pressure, but will stand very considerable pressure. It is impervious to water, though water will convert it rapidly into a demoralized condition. It is ideal material to tunnel in under modern shield methods.

In stating that the work involved all types of tunnel construction, it is to be understood that portions of the work were in solid rock —in certain locations Newark sandstone, and in others gneiss, and later—in part below the waters of the Hudson River, where at times, alongside the American Line Pier, there was a shell of only five feet of rock between the tunnel roof and the river bed—portions in sand and gravel saturated with water, portions in quicksand also saturated, portions in made or filled ground—the most trouble-

some material to work in—portions in river silt, where, theoretically, there should have existed twelve feet of silt cover and where at times there was actually none at all until clay was dumped to make an artificial cover, and that finally the work was carried on in every combination of these materials. Below the Hudson River, where the deep channel of 65 feet of water flowed, the tubes on both the up-town and down-town systems passed from silt to full rock and from full rock to full sand; consequently the sections at every point continually changed.

The essential factors in all tunnel construction may be summed up as follows: (1) The removal and disposition of the material; (2) the support of roof and sides during construction; (3) the elimination or disposition of water entering the work; (4) the provision of a safe place for workmen engaged on construction; (5) the construction of a permanent lining and waterproofing it.

Carlyle, after disserting on the weakness of man, writes: "Nevertheless he can use tools, can devise tools. . . . Nowhere do we find him without tools, without tools he is nothing, with tools he is all."

In no line of engineering work are the tools more essential than in the modern art of tunnelling. In every type of structure and in every material the proper and efficient tools are the prime factor in the advanced new art of this work.

In rock tunnels the combination of the modern compressed air operated rock drill with fumeless high explosives; in soft ground the use of compressed air, mechanical haulage, electric light and power, steel temporary false works and reinforced concrete or iron plate lining; and for subaqueous work the use of a shield and other mechanical appliances, has to-day converted into an essentially mechanical process what at one time was replenished by brute force. The power plant needed for carrying out such a piece of construction is of tremendous extent. For the Hudson and Manhattan tunnels, for example, and exclusive of the Sixth Avenue subway, the combined power plants aggregate some 13,500 horse power boiler capacity, operating high and low pressure air compressors, electric generators, and hydraulic pumps delivering water to the shields at a pressure of 5,000 pounds per square inch, as well as

saw mills and machine shops. Air pressure was maintained continuously from these plants with no break or interruption for support of some part or other of the work under construction, from September, 1902, to June 14, 1909, and during the construction period over 250,000 tons of coal was consumed, while the value of the plant exceeded $750,000.

The plants installed for the Pennsylvania Railroad Tunnel construction were much larger and more valuable than those of the Hudson Company.

The modern method of shield tunnelling is an evolution, and except in the mechanical details and design, is in no respect new.

Referring to the before mentioned essential factors in tunnel construction, the conditions are met by provisions of the tools or means of construction as follows:

1. For the support of the soil, and the elimination of water, and to provide for the safety of the workmen, we carry out the work under air pressure.

2. For the support of the soil, to provide safety for men, for economical and rapid construction, and in certain cases also for the removal of the soil, we install a *hydraulic shield*.

3. For the permanent lining we require material strong enough to stand the external pressures as soon as erected, and therefore a metal *plate lining* is requisite.

4. For putting in place this permanent lining there is installed, either attached to the shield or operating independently, an *erector*.

5. For water-proofing and protecting from corrosion the iron lining, where the same is erected in sand, or gravel, or partially in rock, *cement grout* is pumped into the rear of the lining by a grouting machine.

The use of air pressure was a very early invention. About 1830 Admiral Cochrane, afterwards Lord Dundonald, took out letters patent for use of air pressure applied to caisson or tunnel construction.

The diving bell had been in use at that time quite extensively for laying foundations under water and for carrying on other engineering works. This represents the simplest form of application of compressed air due directly to the water depth, but obviously

without the renewal and replenishment of the air. The greatest possible capacity of bell could only, by any possibility, enable workmen to continue work therein for a very short period, and to renew the air the bell had to be brought to the surface. The admiral conceived the idea that if air were pumped into a working chamber it could be applied at a pressure equal to the full head and thereby exclude water; and, going further still, that if such a chamber were connected with the surface or outer air by a tube or other connection and fitted with large valves, it would be feasible for the workmen to freely pass into and out of the working chamber by simple operation of the valves. This simple method he considered as equally applicable to a caisson or tunnel and for the purpose of excluding water. This is all the air lock is today—a large receptacle of sufficient size for cars, buckets or men, fitted with flap doors, one at each end, and opening the same way, that is inwards, against the higher pressure, and connecting the working chamber with the outer air at atmospheric pressure. It is immaterial whether this air lock is fitted vertical, as is usual in caisson work, or horizontal, as commonly applicable to tunnels, the essential condition being that the lock be built into a bulk-head or diaphragm enabling a pressure of air higher than normal atmosphere to be maintained inside the working chamber, which, in tunnel construction, is usually a length of completed tunnel, while in caisson work it is the lower portion, between the air floor or diaphragm and the cutting edge. The "modus operandi" is extremely simple. To enter air pressure, the person enters through the open outer door or valve and closes it behind him. He is then, so to speak, in a room with both doors shut and at atmospheric pressure. He then admits air under pressure until the pressure within the room is equal to that of the working chamber inside, when, obviously, the inner door can be opened and the passage effected. The use of double and triple locks allowing varying stages of pressure are usual in tunnel work, when high pressures of over twenty pounds are used, as increasing the personal safety of men and expediting the operation in the decompression. The honest observance of rigid rules of health makes the the risks to workmen by air pressure almost nil. These are: (1) Selection of men, limiting the age of men who have not been in the

habit of working under pressure to, say, not over thirty years; and careful medical examination of every employee for physical and organic condition, and the exclusion of highly strung nervous subjects; (2) the honest coöperation of employer with the medical officer, so that no man is employed who has not been passed as sound; (3) the rule that no man should work when sick; (4) the rule that, while the time interval for compression should be reasonably slow, the all-important thing is to limit the time rate for decompression. This is the reason for the use of varying stages of pressure.

In the tunnel work by the Hudson companies, from 1902 to 1907, there were 40,000 men examined for work under pressure up to 42 pounds above normal. In that time, in the tunnel work (not including caisson construction) only three men lost their lives owing to caisson disease. Work in caisson is in this respect more hazardous than in tunnel, even at the same pressures, owing largely to the small capacity possible within the working chamber and the higher temperatures under which work has to be carried on.

It was before mentioned that the original idea of using air pressure was simply to exclude water. Later, when about 1869 Colonel Haskins conceived the idea of our Hudson tunnels, he also evolved the idea that air pressure balancing the external earth pressure would retain it, neglecting the fact that the elastic properties of air allow it to take any form, and that consequently rigid support to maintain the shape of the chamber is essential to the application of air pressure for balancing earth pressures. This oversight was the direct cause of a great disaster which occurred in the early days of the Hudson River Tunnel. The invention of the "shield" by Sir Marc Isambard Brunel, in 1818, marks the most interesting step in the art. He had been asked if it was possible to build a tunnel under the Neva at St. Petersburg, and working out a scheme, took pattern from nature. The *Toredo navalis* destroys wooden piles by boring holes through them just at the mud line. The worm attaches itself to the wood and then bores the hole as it moves forward, lining its hole with a calcareous shell and discharging the borings through its body. Its body also closes the hole against water coming in; for if the worm penetrates the wood, or admits water, it dies,

and obviously, therefore, it is carrying on its boring operations under air pressure equal to the water head.

Brunel's shield comprised a cutting edge and front shell to form the tunnel and hold the sides and roof; the front, or breast, being held by shutters adjustable to the work as excavated. His shield was to be advanced by screw jacks. The modern shield is a great piece of machinery, but in principle is but little different from that of Brunel. The shield is particularly adapted to the construction of a circular tunnel; though roofing shields, simply to support the roof while lining an arch, are not at all uncommon.

Any other form than the circle would be very difficult to use, owing to the trick a shield has of rotating as it advances. The shield structure consists of a drumshell having an internal diameter slightly larger than the external diameter of the finished lining. The front end is shod with very heavy steel castings to form a sharp V-shaped cutting edge protecting and stiffening the shell. The shell is built up of rolled steel plates in two- or three-ply thickness, with all rivets on outside and inside countersunk smooth and no projecting butt straps at joints. This construction gives a smooth external surface to slide through the soil and a smooth flush internal surface in the tail section. The central section of this drum shell is strengthened by a massive girder construction, which forms the main structure of the shield to withstand the pressures and strains. The rear of this girder construction is plated solid and mud-tight, except for doors in each pocket section which can be opened at the control of the workmen, either to admit materials or allow passage for men. This solid plate of the shield is known as the diaphragm. The depth of the girder construction is regulated largely by the length of the hydraulic jacks, which project through the diaphragm and are heeled as near the cutting edge as possible. To provide for the jacks there is constructed within the outer drum shell an annular space, stiffened between each jack by heavy girders connecting the outer shell to the internal jack space shell. The portion of the shield projecting in the rear of the diaphragm, consisting of only a portion of the drum shell (finished flush inside and outside) is known as the "tail." This only serves the function of providing a safe place in the shelter of which to

erect the permanent lining, and slides along overlapping the tunnel tube as construction advances.

If the shield is of large size, there are fitted in front sliding tables, advanced in front of the cutting edge of the shield by hydraulic power, for the double purpose of forming platform for men to work upon while drilling, excavating or breasting the upper face, or to shelter the men working below while doing the similar bottom work, and also to push forward against the timbers used to breast up the face, and to hold them in position during the operation.

The machinery with which the shields are equipped consists of numbers of hydraulic jacks fitted at close intervals around the periphery, the jack cases heeled against and pushing close to the cutting edge by admission of water pressure to the jacks, forcing forward the entire structure of the shield by reaction of the rams against the last erected ring of permanent plate lining. The jacks are controlled by an arrangement of valves, so devised that any jack can be operated singly, or any group of jacks can be operated together by one man, from a convenient platform. By applying pressure to either quarter of the shield, the proper direction is given to advance the shield. The doors are very simple, and the design best adapted depends partly on the character of soil in which the shield is being used. In our work nearly every type of door has been fitted, but the simplest and best adapted to the usual condition of use and emergency is the loose slat. A shield is, needless to say, a very massive piece of construction, as the strains put upon it are enormous. One of the Hudson and Manhattan shields complete weighs about 67 net tons, while a Pennsylvania North River shield weighs about 195 net tons. Presuming that work is being carried out in silt under the Hudson River with pneumatic pressure of 30 pounds per square inch and with all the shield doors closed tightly, the shield is pushed through the clay as though one pushed a stick into a heap of dirt. In that case, with sixteen jacks worked under 5,000 pounds hydraulic pressure, there is a total force of 2,500 tons. This, on the Hudson and Manhattan shields, represents a pressure on the soil equivalent to eleven tons per square foot. It is not often that such a pressure as this is necessary to produce penetration with

consequent flow and displacement in silt, nor could it be used in any stiffer material without probable damage to the shield. Sand or gravel, which will not flow, must be excavated or removed, and removed in advance of the forcing forward of the shield, or injury will be done to the structure. In Hudson River silt the process is very rapid, the complete cycle of operation involved in pushing this shield, cleaning up the silt which leaks through the door joints, placing and erecting a complete ring of plates for lining, bolting up and being ready for the next cycle, requiring only thirty-seven minutes, and in this way as much as seventy-two feet of finished tunnel has been erected in a single day.

In the design of a shield there have been numerous so-called refinements introduced, some patented, some tested and discarded; but in our own experience, the greater simplicity the greater service, and any addition to such a machine which is not necessary it is better without. For example, working in sand or gravel, or when the face consists in part of rock and in part of silt or sand, the entire face of soil has to be excavated, and as this is done the breast must be retained from caving or falling in. This is usually done by skilled men placing boards, well and properly braced by struts, through the shield doors back to the completed tunnel, so that they are independent of the movement of the shield when it is next shoved ahead. The addition to the shield of an elaborate mechanically operated system of shuttering, like a glorified edition of Brunel's idea, is no advantage in time nor labor, but enormously increased cost to construct and additional trouble to maintain.

The most difficult combination to deal with in shield tunnelling is a partial face of rock overlaid by silt or wet sand. This construction was first met with and successfully overcome in the East River Gas Tunnel in New York. In this case the soft ground overhead must be excavated and the exposed face securely supported by timber while the rock underlying is drilled and blasted. This work, carried out in air pressure, necessitates very small charges of dynamite and is very tedious and expensive.

One of our shields with which the east-bound up-town tunnel was constructed from under the Hudson River through Morton Street and Church Street and into 6th Avenue as far north as 12th

Street, travelled a total distance of 4,525 feet, of which amount 2,075 feet was constructed with rock over the partial cross-section of the tunnel, and overlaid with wet sand.

During the progress of this shield and for the purpose of blasting and removing the rock, there were exploded 26,000 sticks of dynamite in front of the cutting edge, causing very great damage to the structure of the shield, so that at the time it arrived at 12th Street and 5th Avenue, the shield was in such condition that it was with considerable difficulty that the tunnel lining could even be erected.

As the shield advances, the permanent lining must be erected in the rear. In Brunel's day brickwork was used, and it has recently been used to some extent, but even with the use of quick setting Portland cement neither brickwork nor concrete is successful for subaqueous work, as the materials cannot reach any strength, in the time during which it is feasible to leave the shield, before advancing it again after construction of a ring. Structural iron, imbedded in concrete at the points where jacks react, has been tried without satisfaction, and the use of wood as a temporary lining, to be backed up after advance of the shield by brick or concrete, while satisfactory for small tunnels, is never likely to be much used for any large sized tubes as needed for railroad tunnels. It is essential, for any tube tunnel construction with a shield, to have a permanent lining which can be erected as the shield advances and rapidly put in place, and which, as soon as erected, is strong enough to take all permanent stresses and to permit of the reaction of the jacks of the shields. A metal lining is the only solution of this problem. Cast iron or cast steel has almost exclusively been used, as the forming of the segments is so accurate, the joints are the least in number, and the material is such as to give the longest life with the least depreciation. If an internal concrete lining is afterwards used, then undoubtedly a satisfactory structural steel lining could be designed, but probably with no economy in cost of construction. We have used this to a small extent for experimental purposes. For any such metal lining, the complete ring has to be erected from within the tail of the shield, which is only a couple of inches larger inside than the external size of the finished tube.

Consequently all the plates cannot be made with radial joints, and a key has to be employed which is inverted from the ordinary keystone form, so that it can be placed in position from within the arch instead of from without. The usual length of the key is commonly equal to the distance center to center of the bolts in the circumferential joints, although often made very narrow like a wedge. The rings used in the Hudson tunnels were two feet long face to face, which means that every ring erected represents two feet of finished tunnel. The rings used in the Pennsylvania tunnels are 30 inches long. The rings are bolted together at close intervals at the circumferential joints by heavy steel bolts, and the segments making up a complete ring, by bolts in each longitudinal joint. The number of segments or plates going to make up the ring depends on convenience of handling, the usual limitation being for a length not exceeding six feet. For the Hudson and Manhattan tubes this worked out to nine segments, all having the same length, and a key. The segments weigh approximately 1,200 pounds apiece, and the placing of these at any point in the circle would be difficult and slow without a machine erector. This is one of the most human-like machines possible. It reproduces the human arm as exactly as possible. The erector (singular or plural, for in a big-sized tunnel more than one erector may be used simultaneously) is a machine either attached to the diaphragm of the shield or entirely independent. We have used both types and have invariably found the independent type most elastic and convenient in operation. This machine may be operated entirely by hydraulic power, or, as is very usual, by hydraulic power for the in-and-out movement of the arm and by pneumatic engine or electric motor for the revolving movement. The end of the arm is fitted with a simple attachment, representing a hand, which grips a plate segment in the middle so that it will balance. The plate is first dumped from a tunnel construction car on to the bottom portion of the tail of the shield already pushed forward and ready to allow the erection of a ring; then the arm of the erector hangs vertically and is attached to the plate; the arm recedes by operation of the hydraulic ram lifting the plate a few inches, then the arm, which is pivoted about its center and partially counterbalanced for the

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weight of the plate, revolves until the plate reaches the position in the circle which it is permanently to occupy, when the arm moves forward, shoving the plate into position, and the bolts are inserted and secured. Plate after plate is erected, until finally the insertion of the key completes a ring, when the shield is again advanced.

By subdivision of all bolt holes absolutely uniformly around the pitch circle in the circumferential joint, it is feasible to shift the position of the key at every ring and thereby stagger the longitudinal joints, increasing materially the rigidity and strength of the tunnels.

For subaqueous tunnelling in silt or sand, this method of the tube construction is essential to safety to employees as well as to safe and certain construction of the work itself; and with it and with competent labor employed, subject to certain limitations as to depth, it is difficult to imagine any condition which would prevent successful execution of a piece of work.

In the tunnel work of the Hudson & Manhattan Railroad there is a greater length constructed under the land than under the water, although every portion, except the Sixth Avenue Subway, north of 12th Street, is below tide level. A portion of this work has been in solid rock, though most of it is built through sand and gravel formation. Under these conditions the use of a shield has not at all times been necessary, and a very large portion of the tunnels could, for economic reasons, be advantageously built with concrete lining, under pressure but without the shield. The function of the engineer is to determine when this can properly be done, and when it can, the saving by substitution of concrete for metal lining is very great, though the metal lining can be more easily made absolutely water tight in wet ground than a concrete lining.

A considerable amount of tunnel construction in solid rock has been carried on with a shield, using iron lining, on account of the likelihood of troubles arising, owing to fissures or thin cover in the roof, which, in the absence of the shield for protection and support, might be troublesome to execute.

The whole art of tunnel construction in soft ground by any process depends on the proper support of every hole exacavated, and as excavated. A small hole in any kind of soil may be safely

made without immediate support; but as soon as it is enlarged, the soil will cave in, unless given support to hold the material *in situ*. This is the purpose of the shield, and it is also the theory upon which tunnels are constructed by the ordinary mining methods. With the latter method air pressure has always been used to keep the water back. Narrow advance headings are driven corresponding to each side wall, and about at the springing line, in which timber mud sills are laid, acting as foundation for the temporary steel sets which support the roof. Then as the soil in the upper portion of the tunnel between side bearings is removed, length by length, these steel centers are put in place to carry the wooden planks known as "laggings," which are placed tight together to give continuous solid support to the external soil. Then the lower portion of the excavation is removed and posts with lagging put in below the sills, to hold the roof and sides until the permanent lining of reinforced concrete can be put in place.

In these subaqueous tunnel operations, as the influx of water is the "bugaboo" of the "sand hog," for the restraint of which air pressure is employed, so the greatest danger arising from the use of air pressure is the "blow out." Safety in such operations is dependent on the maintenance of a nice balance between the air pressure within and the water pressure without the tunnel. In the case of a diving bell or caisson, the exact amount of air pressure necessary to balance the column of water extending vertically from the rim of the diving bell or cutting edge of the caisson to the surface of the water, can be automatically determined. If too much pressure is used, the excess will escape under the cutting edge, and if not enough, the water will rise in the working chamber above the cutting edge. In the case of a tunnel, however, no such exact determination can be had, as the bottom of the tunnel, being deeper, requires a greater pressure to exclude the water than the top, and if this pressure is used, there will not be enough water pressure at the top to prevent the air from escaping, and if only enough is maintained to balance the water at the top, the bottom will be flooded. Fortunately the overlying material, to a greater or less extent, dependent on its character, permits of the maintenance of a greater pressure than that due to the hydrostatic head, so that the

tunnel engineer exercises his judgment and fixes on a pressure that will not escape through this material and will exclude as much water from the bottom of the tunnel as possible. Some escape of air occurs in nearly all materials, excepting the most impervious, and this escaping air, if pressure is too great, blows the particles of materials away and enlarges the passages until they become so large that the air escapes faster than it can be supplied, when the air pressure drops and an inrush of water occurs. This is what is meant by a "blow-out."

For any railroad tunnel, in order to minimize the gradient of approaches, the grade is necessarily established at the least depth feasible below the bed of the river or below the surface; and where, as in the Hudson River, the water is sixty-five or seventy feet deep, and man cannot, with safety, work under air pressure exceeding forty-five pounds per square inch, which represents a hydrostatic depth of salt water of one hundred and two feet, it is obvious that the amount of cover over the roof of a tunnel and below the bed of the river must be very small. If the soil is in any degree porous, and pressure is allowed to drop, there is a grave liability of the ground becoming "demoralized" by infiltration of water, and if pressure is then raised to check the inflow, it sometimes happens that the cover of soil cannot withstand the increase, and the roof is blown off or a big hole blown out, and water then comes in in large quantities, often beyond control, so as to flood the tunnel works within the air locks.

The instinct of an unskilled foreman is usually to raise pressure in case of a leak commencing. If nothing happens it may be a good thing to have done, but an expert will usually lower the pressure in this case, putting up with the nuisance and difficulty of having a good deal of water in his tunnel and thereby allowing the greater external pressure to squeeze what soil exists into the pockets of the shield and thereby choke the leak. There is far greater skill and caution needed to raise air pressure than to lower it, in case of any leakage occurring. In the event of a "blow" it may be small, in which case it can be stopped from inside by stuffing up into the hole bags of sawdust, hay, balls of clay, and in fact anything handy and available to fill a hole. Usually this prompt

treatment will relieve the condition so as to allow the shield to be pushed past the blow hole. Sometimes, however, the hole will increase in size, often quite suddenly, and get beyond control, in which case all doors in the shield are closed up and the men are removed before the tunnel fills with water. In one famous incident which occurred in one of the East River tunnels, a small blow had developed and a man was in front of the shield stuffing bags of hay in the hole, when the pressure sucked the bag into the hole suddenly, and the man, failing to let it go quickly enough, was carried, hanging to his bag, through the ground into the waters of the river, when he rose to the surface and was picked up alive by a passing boat. When the condition is reached that the tunnel is completely flooded, the steps to be taken to recover it must be from outside. Soundings are then taken very carefully and accurately to determine the locality and extent of the blow hole in the bed of the river. A large quantity, usually several hundred cubic yards of good stiff silt or clay, is loaded in a dumping scow, and at "slack water" the scow is towed into position over the hole, being located by instrumental observation from triangulation points on shore, and on signal from the two observers of simultaneous correct position the scow is instantly dumped. Soundings are then again taken to ascertain if the hole is completely filled, and if not, scow after scow is dumped at the same point. Following that, air is then pumped into the tunnel, forcing the water out through the blow pipes; as the pressure is gradually raised it soon becomes evident whether or not the hole is plugged. In the construction work on the up-town tunnels of Hudson and Manhattan, there occurred fully a dozen blows, necessitating dumping clay, although only in four or five cases was the tunnel flooded. In one of these most persistent cases, which had been occasioned by gross incompetence on the part of the night foreman, who advanced the shield with too many doors open, dumping was carried on until half the tunnel was full of silt, and then the leak was only stopped by spreading a sail sheet over the hole and dumping additional material on top. When the tunnel was recovered and cleaned out, the sail covered the door opening. The increased skill and experience attained by the workman before the down-town tunnels were built was shown in the fact that only

two blow-outs occurred, in neither of which cases was the tunnel lost, and each of these was due to striking fissures in the rock formation close in shore. It is not unusual to provide against the chances of "blow outs" by artificially increasing the cover over a tunnel by dumping good stiff clay as a thick blanket over the tube location before the work is executed, and ultimately dredging and removing this clay. This method was carried out, to a very large extent, in the East River tunnels of the Pennsylvania Railroad.

One important item of work in connection with subaqueous tunnelling is that of "grouting." The purpose of this is two fold: (1) To fill the small annular space due to the shield's being slightly larger than the tunnel lining, as well as any other voids or cavities in the rear of the working face, and render the exterior soil solid, thereby giving more perfect support to the lining; (2) to waterproof and protect the material of the lining itself. Grouting consists in mixing hydraulic cement with an excessive quantity of water, so as to make it almost liquid, and then pumping it, either with a force pump or grout machine, through holes provided in the lining, into any spaces or interstices in the surrounding material. Cement mixed neat or in a very rich mortar with sand, and particularly if it has originally been mixed with an excess of water, when set up and thoroughly crystallized, makes an exceedingly impervious material. In the iron lining plates, there are usually provided one hole to each segment of plate, screw-tapped to permit the insertion of a piece of iron pipe having a hose attachment connected to the grout machine. This machine was invented by Greathead, and consists of a small tank in which blades or paddles are rapidly revolved on a shaft by an air engine or motor. To the tank is fitted an extension box with a flap door opening inwards, through which the change of cement (usually one barrel at a time) with the water needed to make slurry of it, can be admitted. The only other attachments are a large pipe for admission of air at high pressure, and a discharge pipe fitted with a cock. The operation is simple. Having put into the tank the cement and water, the paddles are run until they are thoroughly mixed, the flap door closed, and air admitted at high pressure into the tank. As soon as the discharge valve is opened, the mixed grout is forced through the hose to its

position outside the lining. The penetrating power of grout under high pressure is remarkable, and it will follow the easiest channel. It is not desirable to use grout where the exterior soil is silt, as the pressure of water softens the silt into mud, preventing the setting of cement and demoralizing the silt in proximity to the lining. Further, it is not required in that character of soil to fill the voids, as clay will flow sufficiently to fill all voids itself. In sandy soil, either with metal or concrete lining, grout is always used, and usually in the case of tunnel built in rock. Along the streets of New York we have repeatedly traced dead connections with sewers by finding grout follow open pipes into houses and fill the cellars. Lifting the tracks of railroads or lifting asphalt pavements of streets sixty feet overhead of the tunnel, have not by any means been unknown occurrences, and the bodily raising of a standing building has been done on one occasion. By filling all voids promptly as the shield progresses, when tunnelling under land, the settlement of soil can be reduced to an extremely small amount, so much so that short lengths of tunnel have been driven under occupied dwellings without the tenants being aware of the fact.

The subject would hardly be complete without a brief reference to the construction of portions of the tunnel work by caisson methods. The general underlying principles of caisson construction are similar to those of shield tunnel work—progress being made vertically instead of horizontally—but for a great many years the use of caissons has been almost exclusively for the construction of piers for bridges, or where solid support is necessary for building foundations.

In locating the tunnels for the Hudson and Manhattan system, where the north and south line connecting the steam railway terminals on the New Jersey side intersect the up-town pair of river tubes, a complicated problem existed as to how to connect these lines, so as to enable the trains crossing the river to run southerly to the Pennsylvania and Erie Railroad terminals, northerly to the Lackawanna terminal, and from the latter point to the down-town river tubes. To enable these train movements to be executed necessitated what in railway parlance is known as a " Y " junction. In case the tracks for trains moving in opposite directions had been

placed side by side at the same elevation, as is usual, dangerous grade crossings would have been established at three points of juncture, which would have impaired safe and rapid operation. The idea was therefore conceived of placing the tracks for trains moving in opposite directions close together, but one above the other, so that at points of intersection they would be on different levels. At the three junction points where each single tunnel diverged, it was necessary to build a switch chamber or section of tunnel, having the width of a single tunnel at one end and gradually widening until sufficient width was obtained for the construction of the two diverging tubes; and owing to the separation of grades in the tubes, these chambers needed to be double decked. As the shield is only adapted to the construction of a fixed size and form of tunnel, it could not be used, and some other method had to be devised.

To construct these chambers by underground mining methods involved very serious hazards, due to their great size and the unstable character of the material at the roof level. On the other hand, to dig an open pit from the surface was not feasible, owing to the great depth and danger of infiltration of water from the river nearby into the excavation. The method finally adopted was to build these sections of tunnels on the surface as monoliths of concrete reinforced with steel, and then to sink them as pneumatic caissons to their final position. They were built complete with the exception of the bottom of the lower chamber, which was left open so as to serve as a working chamber for the excavators. The material excavated was passed out through shafts extending through the upper chamber, and thence through an air lock to the surface, whence it was removed or dumped on the descending caisson in order to give additional weight to cause the caisson to sink by overcoming the friction of the surrounding material on the sides. When each caisson had been sunk to the proper level, they were completed by an inverted concrete arch closing the bottom and sealing the connecting tunnels joined to them. In some cases the shields were driven to the caissons, then rolled through and continued on the opposite end. Each of these caissons took about three months to sink, and the largest weighed twelve thousand tons. During the construction and sinking, by arrangement with the rail-

road companies, traffic was suspended on the railroad track immediately above the caissons.

The conditions were very much worse, however, in the case of the construction of the approaches to the Church Street Terminal Station under Cortlandt and Fulton Streets, where the arrangement of switches desired was such that the tunnels could not be built by shields and the caisson method was used. In this case the business had to be continued on the surface of these streets at all times, from beginning to end of the construction work, and further, all sewer pipes, steam pipes, electric conduits, etc., had to be in use and operation throughout the entire period of construction. The difficulty, due to adjacent buildings not having foundations which extended below the cellars, made necessary other methods than those adopted in Jersey City. The cofferdam enclosing the entire area of the buildings had already been sunk, by a series of caissons, to bed rock around the entire area, and these approaches had to be constructed contiguous with the exterior of the cofferdam taking in practically the entire width of Cortlandt Street and Fulton Street, building line to building line.

In this case an absolutely new departure in caisson construction was adopted, which involved sinking boxes, forming short sections of tunnel, with solid sides, the same being built up as the caissons descended. These boxes had to be sunk entirely from below the level of the street, and their sides were built up five feet at a time as they sank, special types of air locks being necessary to enable the work to be carried on in this manner. These caissons, too, contrary to the usual custom, were sunk without roofs, that is to say, the permanent floor of the tunnel was designed as the roof of the working chamber, and the sides of the box built up of reinforced concrete, but the ends with steel plates reinforced with pin-connected truss girders. These boxes were sunk by loading with pig iron in order to obtain the necessary weight, and when they had reached their permanent position timber struts were put in to take the external pressures on the side walls, pending the construction of the roof. When the various caissons had been sunk, one against the other, until the whole strip of tunnel had been completed, the

removable steel ends of the boxes were taken out and the permanent roof put on to the side walls.

The difficulties of doing this work under the conditions may be appreciated when it is understood that, in Cortlandt Street alone, the 6th and 9th Avenue Elevated Roads had to be supported, and not only a public sewer in each street had to be maintained, but also a twenty-four inch steam pressure main (under 90 pounds of steam pressure all the time) as well as a bank of electric conduits which carried about seven thousand wires, comprising every telephone and telegraph wire in the down-town section going out of New York City.

The tunnels under these streets could undoubtedly have been built by the tunnel methods indicated in the earlier part of this paper, possibly at some saving in cost, but any such methods would have involved such risks of damage to buildings and adjacent properties that such a saving would have been far more than offset by damages in case a building was wrecked. By this method the construction of these difficult portions of tunnels, where junctions between the different lines were involved, could be carried on with almost absolute safety, and as the results proved, there was practically no injury to any properties adjacent or immediately above the construction itself.

The work which has recently been carried out in New York City, the construction not only of the tunnels of the Hudson and Manhattan Railroad, but also those of the Pennsylvania Railroad, have developed almost every possible combination of conditions which could by any possibility arise in tunnel construction, and the enormous magnitude of work carried on in the last six or seven years has been done with practically no disasters of any kind. These methods described have brought the modern construction of tunnels down to almost an exact science. The hazards and dangers are infinitely less than in the case of bridge construction, and tunnels are applicable in many cases where bridges are not.

In addition to this, the different tunnels permit very much greater elasticity in the development of transportation facilities than the bridges do, and traffic can be more economically and better distributed by developing underground routes than by construction of

immense bridges which, on account of the lateral strength and rigidity, must be built on an enormous scale, whereas tunnels can be constructed on any lines, wherever they will give the greatest facilities for distribution of the travelling public.

New York City, and in fact all of our great cities, are absolutely in their infancy in respect to the tunnel for transportation. There is practically no limit to the development of this line of work as the necessities arise and as traffic demands, and we have reached in New York City a point where the possible construction of tunnels can hardly, by any possibility, keep pace with the growth and development of the population and with the necessities for their transportation.

In respect to the rivers, the future, unquestionably, has within sight construction of highway tunnels between New York and New Jersey on the lines of the Blackwall under the Thames in London or similar tunnels under the Clyde at Glasgow, forming underground extensions of existing street thoroughfares; and the object of this presentation of the subject is to give an accurate idea of the principles underlying the construction of such arteries of transportation.

A BRAIN OF ABOUT ONE-HALF THE AVERAGE WEIGHT FROM AN INTELLIGENT WHITE MAN.

By PROFESSOR B. G. WILDER.

(*Read April 22, 1910.*)

For the privilege of examining and reporting upon this unusual —perhaps unique—brain I am indebted to Dr. J. H. Larkin, professor of pathology in the College of Physicians and Surgeons, New York city.

History.—According to Dr. Larkin's records Daniel Lyon died on the tenth of October, 1907, from asphyxia due to edema of the glottis. He was Irish, 46 years old, five and one-half feet high, and weighed 145 pounds. No relatives have been discovered and it is not known that any survive. At the time of his death he lived at 409 E. 17th St., New York City, and was a watchman for the New York Contracting Company at the Pennsylvania Terminal, 34th St. The legal representative of that company says that " from all reports there was nothing defective or peculiar about him, either mentally or physically." No photograph or hat-measurement has been obtained. No information has been gained by inquiries addressed to his alleged fellow-workmen or former places of residence, but Dr. Larkin was informed that he could read and write; that he was regarded as competent and in full possession of his faculties; and that as a laborer he had worked in one position for twenty years. There seems to be no reason why he should not be regarded as of ordinary intelligence; yet, as will be seen, his brain might have belonged to a feeble-minded person, or even an idiot.

Shortly after death the brain was removed in the presence of Dr. Larkin and the coroner's physician, Dr. Philip O'Hanlon. No head-measurements were made, but it did not appear to be unusual in either size or shape. The brain filled the cranium; there was no

excess of liquid, and no evidence of compression. Upon accurate scales the brain was found to weigh exactly 24 ounces, or 680 grams, about one-half the average for male Caucasians. It was placed immediately in ten per cent. formalin, and there remained until sent to me more than two years later.

Examination and Results.—At its reception by me on the thirteenth of January, 1910, the brain weighed 714 grams, having enlarged slightly in the preservative. Subsequent immersion in alcohol reduced the weight to 682 grams on February 7 and on April 6 to 512, a trifle over 18 ounces. Of this total, after transection at the midbrain, the cerebrum proper represented 404 grams and the cerebellum, with the pons and oblongata, 108; the ratio is less than 4 to 1 instead of the usual ratio of about 8 to 1. Indeed, the cerebellum seems nearly normal in size and form, while the cerebrum lacks about one-half the usual weight and is peculiar in several respects.

Form of the Cerebrum.—The present disproportionate width and the flatness of the middle of the dorsum are probably due to the weight of the brain itself as it hardened in a liquid of less specific gravity. To the same cause may be ascribed some of the divarication of the occipital lobes and the concomitant exposure of the cerebellum; respecting the original conditions in these respects there are neither records nor recollections. The occipital lobes are very slender, and their cavities are slight. The cunei, associated with vision, are extremely narrow, but the parts of the temporal lobes associated with smell are well developed, and the postrhinal fissures distinct. On both sides the insula is partly visible, but that sometimes occurs with ordinary brains, and in the superior brain of a philosopher, Chauncey Wright. On the left the insula has been fully exposed by removing the operculums; it is small, presents no true fissures, and resembles a rounded ridge curved sharply about a deep pit at the dorsal side. The precunei, regarded as "association areas," seem fairly well developed. Whether there is a marked deficiency of the "speech center," the subfrontal gyre (Broca's convolution) I am not yet prepared to state. Transections of the right hemicerebrum at four levels indicate unusual smallness of the cavities, and a reduction in the alba

or white substance rather than in the cortex or gray matter; upon these points and upon the size of the callosum I hope Dr. Spitzka may be willing to comment even at such short notice.

Fissural Peculiarities.—Of these there are several, and some are indicated upon the charts. Their detailed discussion would be out of place in a comprehensive society like this until such time as the normal human fissural pattern is familiar to pupils in or below the high school.

This brain is not ape-like. Even were it still smaller it is distinctly human. The mesal cleft or "valley" of the cerebellum is deeper than in any ape. The calcarine and occipital fissures are deeply continuous while, with rare exceptions, they fail to unite in apes. Even the insula, deficient as it is, does not resemble that of apes, and the "speech-center" is of the human type.

Brain-weight and Intelligence.—Upon the present occasion attention is particularly directed to this exemplification of the possibility that ordinary human intelligence may apparently coexist with a brain of only one-half the ordinary size, exceeding that of certain apes by only 180 grams (about six ounces), and not quite double the size of the brain of a congenital idiot.

DERMAL BONES OF PARAMYLODON FROM THE ASPHALTUM DEPOSITS OF RANCHO LA BREA, NEAR LOS ANGELES, CALIFORNIA.

By WILLIAM J. SINCLAIR.

(*Read April 22, 1910.*)

In the excavations conducted by the Department of Palæontology of the University of California in the Pleistocene asphaltum deposits at Rancho la Brea, near Los Angeles, large numbers of small bones have been found, resembling closely the dermal ossicles described by Dr. A. Smith Woodward (2, 3) from a piece of the skin of a gravigrade edentate, *Grypotherium listai,* found in a cave at Last Hope Inlet, Patagonia, and also resembling the previously known dermal bones of *Mylodon* (1).

In the asphaltum, two edentates have been found so far, one apparently related to *Megalonyx* and the other referable to the genus *Paramylodon.* As dermal bones, among the Gravigrada, are known only in the Mylodontidæ, there is every reason to regard those presently to be described as pertaining to *Paramylodon.* Comparison of a skull and jaw from the asphalt with the figures of *P. nebrascensis* Brown (4) from the Pleistocene locality of Hay Springs, Nebraska, seems to indicate that they should be referred to this species.

In a preliminary paper on the Rancho la Brea deposits, Professor Merriam (5) says, regarding the discovery of ossicles:

> During the first examination of the beds several small, pebble-like bones were obtained which resembled the dermal ossicles of the ground-sloth, *Grypotherium,* recently described by Dr. A. Smith Woodward from skin fragments obtained in a cave at Last Hope Inlet, Patagonia. The ossicles were in association with remains of a large ground sloth somewhat similar to *Mylodon* in foot structure. Realizing that the peculiar conditions of accumulation offered an especially favorable opportunity for preservation of the dermal armor of a ground-sloth, during the second study of the deposits an attempt was made to find a specimen in which the armor might be recognized. Several hundred yards from the location of the first specimen, a large scapula

resembling that of a mylodont was found partly exposed, with a row of small ossicles immediately over the outer side. The section of the bed containing these bones has recently been worked out, and the row of small bones proves to be the edge of a distinct layer including between 250 and 300 individuals. They mantle over the outer surface of the scapula, being removed from it by about an inch of asphalt.

The layer of bones as we find it has probably been disturbed somewhat and does not occupy its original position exactly, but the fact that it remains as a distinct layer with a tendency toward similar orientation of the individual ossicles indicates that the disturbance has not been great. As the position of the layer in the asphalt was nearly vertical, the presence of the large number of ossicles together may not be attributed to the washing together of scattered elements on the floor of a small basin of deposition.

The ossicles are not closely pressed together and are not superimposed. The individuals range in size from a cross-section of 6.5×4.5 mm. to 21×16 mm. Excepting a few of the largest ones, which are nearly square, the greater number are rounded and rather irregular in form. The outer side is in some cases more regularly modeled than the inner. The surface of the bones is somewhat roughened or pitted in some instances, but no markings are present which would be considered as definite sculpturing. The microscopic structure has not yet been examined.

In general the form, size and arrangement of the ossicles are much as in the bones in the *Grypotherium* skin from Patagonia. The skin fragment first described by Woodward was thought to represent mainly the region of the neck and shoulder. The Californian specimen mantles over the outer side of the scapula, and is presumably not far removed from its original position with relation to this bone.

Subsequent investigation has added little to this, so far as the localization of particular types of ossicles is concerned. Some idea of the diversity of forms assumed by them may be gathered from the accompanying figure, in which several of the types mentioned in the citation may be recognized (especially in Fig. 1, *b*, *c*, *d*, *g* and *h*), nor do these differ essentially from the great mass of scattered and unlocated ossicles. Pitted and smooth forms occur in the same individual. In some of the bones, except for minor undulations and the pores for the entrance of blood vessels, the outer surfaces are smooth and polished (Fig. 1, *e*, *f*). Others show a highly irregular pattern of small anastomosing ridges (Fig. 1, *b*), but, as previously noted, there is no constancy or regularity in the pattern. Grooves cut across some of the ossicles, as shown in the figure. Some of these may be due to the fusion of two adjacent elements, as suggested by Woodward for the origin of a similar structure in *Grypotherium*.

Thin sections of the ossicles of *Paramylodon* were submitted to Professor E. G. Conklin who has kindly examined them and furnished the following note:

Histological Character of Dermal Bone, Horizontal and Vertical Sections.—Penetrated by many canals for blood vessels, which, in general, begin perpendicular to the surface but soon branch repeatedly and frequently anastomose. Occasionally Haversian systems of lamellæ and lacunæ may be seen around these canals, but generally these lamellæ and lacunæ are very irregularly disposed between the canals. One of the most striking features is the presence of bundles of fibres which run in all directions through the bone, making the latter look almost like a woven fabric. These fibres are most abundant in the spaces between Haversian systems.

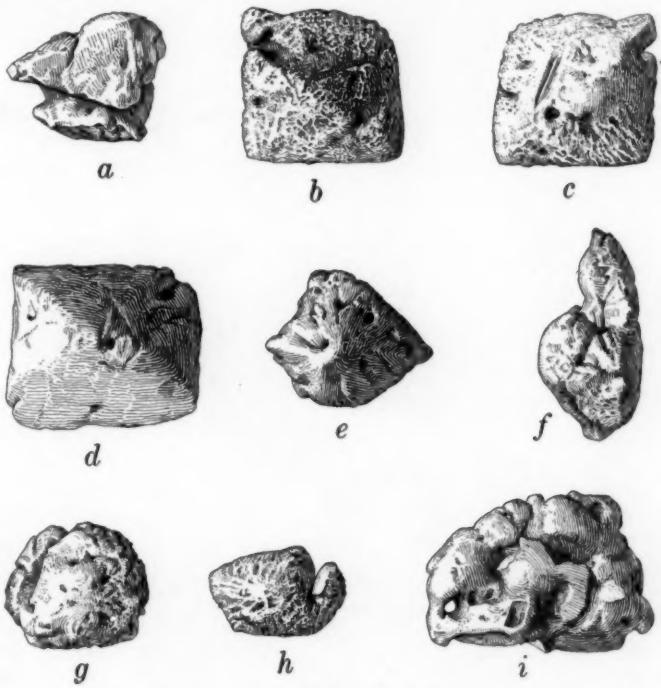

FIG. 1. Dermal ossicles of *Paramylodon nebrascensis* showing some of the various forms assumed. *a*, irregularly shaped ossicle with transverse groove; *b*, *c*, rectangular ossicle with groove toward one corner; *d*, another rectangular bone with pyramidal surface; *e*, *f*, ossicles of irregular shape, similar to some of those found overlying the scapula; *g*, *h*, ossicles of the same character as those overlying the scapula; *i*, pitted ossicle, outer (?) surface. All the figures are one and one half times the natural size.

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Comparison with Woodward's figures of the histological structure of the ossicles in *Mylodon* and *Grypotherium* (2, Pl. xv., Figs. 7, 8) shows a close agreement between the latter and Paramylodon, the only differences appearing in the absence of all suggestion of a zonal arrangement of the lacunæ toward the lateral margin of the bone and the less frequent occurrence of Haversian systems, which are best observable in a transverse section of one of the *Paramylodon* ossicles. In all other respects there is the closest agreement, both in the presence of interlacing fibres traversing the mass, the absence of radiate structure at the periphery, and the development of Haversian systems about the vascular canals, features which are not developed in *Mylodon*.

In addition to the acknowledgments already made, the writer desires to express his indebtedness to Professor J. C. Merriam to whose kindness is due the opportunity to examine and describe this new material.

PRINCETON UNIVERSITY,
April, 1910.

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THE RESTORED SKELETON OF LEPTAUCHENIA DECORA.

By WILLIAM J. SINCLAIR.

(*Read April 22, 1910.*)

A recent examination of the *Leptauchenia* material in the Princeton University collection has made possible the presentation of the accompanying restoration of the skeleton (Fig. 1), together with some notes on the structure of this animal. Parts of two species are represented, referable apparently, to Leidy's *L. decora* and *nitida* respectively. All the specimens were collected some years ago by Mr. J. B. Hatcher at Corral Draw, in the White River badlands of South Dakota.

In a general way, there is a good deal of resemblance between the writer's restoration of *Leptauchenia* (Fig. 1) and Peterson's reconstruction of *Phenacocœlus*,[1] an animal about one fourth larger, presumably related both to the first mentioned genus and to *Cyclopidius*. The major portion of the skeleton is drawn from two individuals of *Leptauchenia decora* (Nos. 10753, 10773 Princeton University collection) supplemented occasionally by other specimens of the same species. The fore foot is from a somewhat smaller individual of *L. decora* (No. 10770) while the hind foot, with the exception of the tarsus, is enlarged to scale from *L. nitida* (No. 10765). On the whole, the restoration recalls to mind an animal of somewhat pig-like proportions due to the large head, short limbs and short tail which may have been longer than indicated. No certain conclusion can be drawn from the skeleton as a whole regarding the habits of the animal. That it was aquatic inferred from the prominent rim of the auditory meatus implying a "valvular closure of the organ of hearing" to prevent the inflow

[1] Peterson, O. A., "The Miocene Beds of Western Nebraska and Eastern Wyoming and their Vertebrate Faunæ," *Annals of the Carnegie Museum*, Vol. IV., No. 1, p. 32, Fig. 5, 1907.

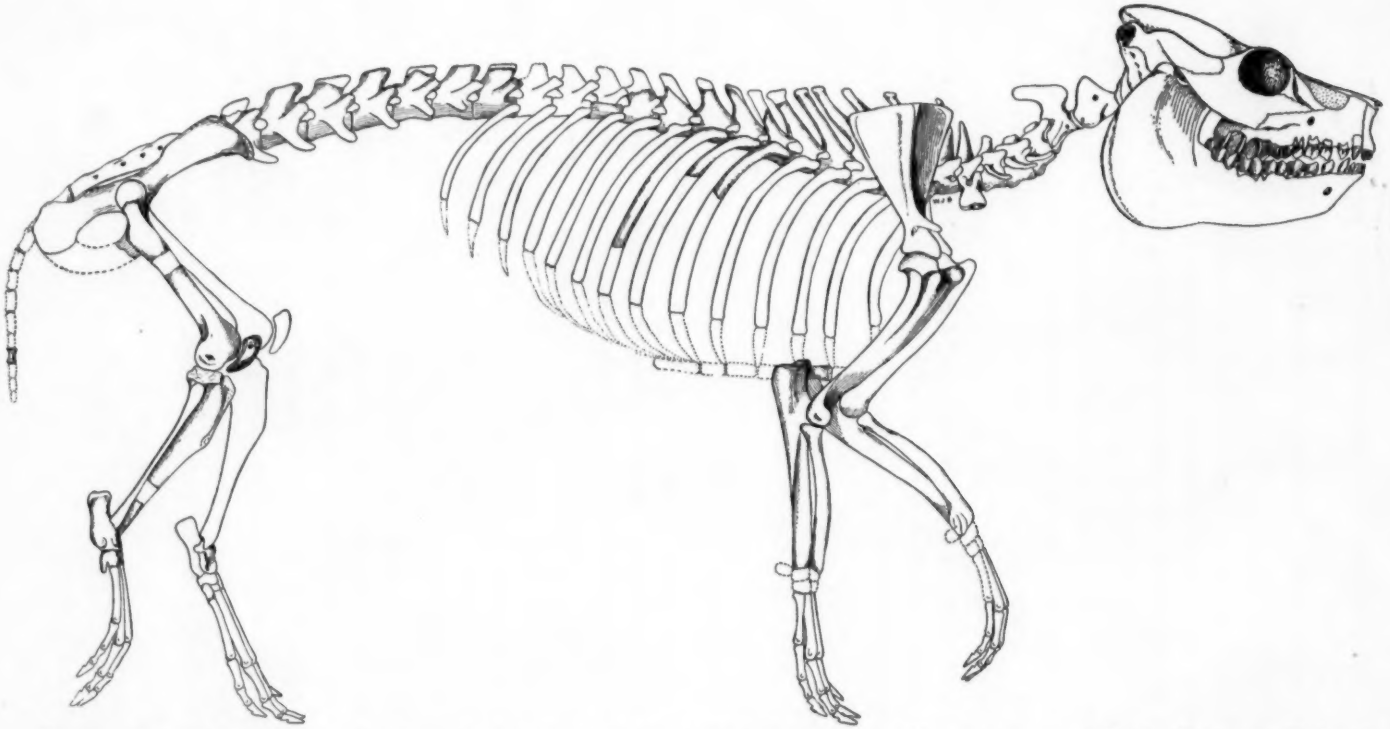


FIG. 1. *Leptauchenia decora.* Restoration of the skeleton based on two specimens (Nos. 10753, 10773) in the Princeton University Collection. Unshaded parts are supplied from other specimens or enlarged to scale from *L. nitida.* Broken lines indicate hypothetical reconstruction. One third the natural size.

of water as suggested by Cope for *Cyclopidius*[2] is not altogether substantiated by the feet, the slender toes of which terminate in small hoof-like elements well adapted apparently, so far as their structure is concerned, to running on firm ground. The slight development of lower incisors and canines indicates, perhaps, that *Leptauchenia* was not a grazing animal, for in modern grass feeders, while the upper incisors and canines may be absent, the lower teeth are broad and flat, well adapted to cropping grasses, while in *Leptauchenia* they are almost cylindrical. A study of the conditions of sedimentation involved in the accumulation of the so-called clays in which the remains of *Leptauchenia* occur will, it is believed, afford a safer clue to its habits than does the anatomy.

As it has not been possible to have detailed drawings prepared it has seemed advisable to omit full description and present merely some notes on the general structure of the skeleton.

Skull.—The prominent orbits, the large facial vacuities extending backward between the eyes, the high and almost straight sagittal crest, the elongated auditory meatus with thickened lip, the heavy arches and the deep mandible are at once apparent from the figure. Coupled with these as generic characters are the enormously expanded auditory bullæ and the reduced condition of the anterior dentition. Some differences in proportion appear between this and a previously published figure of the skull of *Leptauchenia*,[3] due probably to the fact that the latter is a composite.

Vertebral Column.—The dorso-lumbar vertebral formula is twenty, of which fourteen are dorsals. Six vertebræ are coössified in the sacrum, three of them being in contact with the ilium. As shown in the restoration, the anterior dorsals have high narrow spines, sloping backward. These decrease in elevation posteriorly and probably about the twelfth or thirteenth dorsal begin to assume the shape of the wide, transversely flattened lumbar spines. A few proximal caudals are preserved, too few to determine with certainty whether the tail was long or short, but suggesting the

[2] Cope, E. D., "Synopsis of the Species of Oreodontidæ," PROC. AM. PHIL. SOC., Vol. XXI., p. 547.

[3] Scott, W. B., "Beiträge zur Kentniss der Oreodontidæ," *Morphologisches Jahrbuch*, Bd. XVI., Taf. XV., Fig. 15.

latter from their comparatively small size. In the cervical series, the atlas is characterized by a slender inferior arch and broad superior arch with strong backward slope to its dorsal profile. The canal for the vertebral artery perforates the base of the transverse process at the margin of the posterior cotylar surface, where it is inclosed by but a narrow bridge of bone, soon emerging on the lower surface of the process. Some distance anterior to the point of emergence, it again perforates the transverse process, joining the neuro-arterial canal near its point of emergence. The margin of the transverse process is broken in all the specimens examined, but seems to have been circular. The axis carries a large neural spine, of which the dorsal border slopes strongly backward and upward. All the transverse processes of the cervicals are perforated by the vertebral artery.

Girdles.—The scapula is a triangular element of which the outer surface is divided unequally into large post- and small pre-spinous fossæ by the prominent scapular spine, of which the acromion process is directed forward. Of the pelvic girdle, the ilium is broadly expanded in the transverse plane, rather more so than the figure would indicate. The gluteal fossa is shallow and the anterior superior spine quite prominent. The ilio-pectineal eminence varies in prominence in different specimens. Both ischial and pubic rami are stout. Their distal expansions are lacking in all the Princeton specimens.

Limbs.—The strong forward curvature of the radius and heavy olecranon process of the ulna are perhaps the most striking features of the fore limb. Apart from this, there are no peculiarities which call for special comment. It is possible that the lateral digits in both fore and hind foot have not been given sufficient length, the error, if such it be, arising from an attempt to scale the drawing from a few dissociated phalanges. But one terminal phalanx is represented in the Princeton collection, and of this the tip is broken off and the margins somewhat damaged. It seems to have been a small pointed hoof.

PRINCETON UNIVERSITY,
April, 1910.

ANTARCTIC GEOLOGY AND POLAR CLIMATES.

By WILLIAM MORRIS DAVIS.

(*Read April 22, 1910.*)

It seems desirable at the present time of active interest in Antarctic exploration to call attention to a point that deserves the special scrutiny of geologists who may visit far southern regions.

Exploration already accomplished has shown that the Antarctic as well as the Arctic lands contain geological formations indicative of a much milder climate than that which prevails in high latitudes today. Thus far, the evidence of mild polar climates has been drawn almost exclusively from fossils or land plants contained in stratified continental deposits. The structure of non-fossiliferous continental formations at high latitudes has not been minutely studied in their bearing on climatic problems. Investigations of recent years in temperate latitudes have however shown that the detailed structures of land-laid stratified deposits may also be used with much success in determining the climate under which they were formed. The studies of Professor Joseph Barrell, of Yale University, published in the (Chicago) *Journal of Geology* for 1908, deserve especial mention in this connection; for they have clearly set forth the characteristics of continental formations in contrast to marine formations, and they have further suggested a variety of tests by which the climate under which continental deposits were formed may be inferred. Under an ordinary or normal climate, neither glacial nor arid, land-laid stratified deposits are chiefly the work of aggrading streams, and as such they will be characterized through the greater part of their mass by frequent and irregular changes in texture, with cross-bedding, lateral unconformities, red color, ripple marks, rain prints and mud cracks. Evidently then the detailed structure of continental formations in high latitudes may be nearly as significant of mild climate as is the occurrence of fossil land plants. Furthermore, the formation of

continental deposits is greatly favored by large continental area; and conversely, large continental area may be inferred from an abundance of continental deposits; thus the problem of polar climates is linked with the equally interesting problem of the changes in land and sea areas through geological time.

There is an important theoretical matter to be mentioned in this connection. It will be remembered that Professor T. C. Chamberlin presented a communication at one of the April meetings of this society several years ago, in which he suggested that a mild polar climate might be caused by a reversal of the deep oceanic circulation, whereby the warm surface waters of the torrid belt would sink, creep along the bottom toward either pole, and rise in high latitudes, where their warmth would determine a climate very much milder than that of today. Evidently if this or any other process that is capable of producing a mild polar climate has been in operation at one time in the past, it may have been in operation at various other times; and thus a question rises to which Mr. Bailey Willis drew attention in his address before the geological section of the American Association at the Boston meeting of last winter; namely, what has been the prevailing climate of the polar regions through the geological ages? Naturally we open this inquiry with a predisposition to regard the climate now prevailing in high latitudes as the normal climate; but if it once be shown that a mild climate has sometimes prevailed there, it is entirely possible that a mild climate and not the rigorous climate of today really represents the prevalent conditions of the polar regions through geological time. Under Chamberlin's theory of mild polar climate, rain would be abundant but mud cracks would be rare; hence even so small a detail as the relative proportion of these minute structures in continental formations of high latitudes would have its significance. Marine formations will probably give less decisive evidence in this respect than continental formations; but marine formations would also have their importance, not only by reason of the fossils they might contain, but perhaps even more from the presence or absence of scattered boulders and gravels, such as might be dropped on a sea floor from floating icebergs. The prevailing absence of such intermixture of floated materials in the marine for-

mations of former polar sea would be almost as significant as the occurrence of fossil land plants in the continental formations of high latitudes.

It is certainly very suggestive that the stratified formations of high latitudes have already repeatedly yielded evidence of mild climates, chiefly as above noted in the form of fossil land plants contained in continental formations, partly in the form of red sandstones; and it is certainly striking that little or no evidence of ancient glacial climates in the polar regions has been found either in continental formations in the form of tillite lying on striated rock-floors, or in marine formations in the form of coarse materials scattered through fine-textured sediments. The inference thus warranted in favor of not infrequent mild polar climates ought to be followed up by critical examination of all pertinent evidence, such as the detailed structure of non-fossiliferous continental formations in high latitudes may furnish.

Antarctic exploration is of particular importance in this respect; for the Antarctic regions are today at least of continental habit, in contrast to the Arctic regions which are, on the other hand, of oceanic habit. Speculations are already abundant as to the formerly much greater extension of Antarctic lands, so as to form connections with other continental masses; and on the present remnant of so greatly extended an area, the possibility of finding continental formations is increased. It therefore seems fitting to bring this matter before that Society which, more notably than any other in the United States, has recently taken active steps in promoting American participation in Antarctic work; with the suggestion that it should be presented to the attention not only of American geologists who undertake southern voyages, but also of the geologists in expeditions sent out by other countries. The problem thus offered for investigation may fairly be regarded as one that has far-reaching results; for if it should appear that the earth's polar climates have really been prevailingly mild, we should have to frame new conceptions of terrestrial physics.

CAMBRIDGE, MASS.,
April, 1910.

THE PROPAGATION OF EXPLOSIONS IN MIXTURES OF PETROLEUM VAPOR WITH AIR IN TUBES.

By CHARLES E. MUNROE,

Professor of Chemistry, George Washington University, Washington, D. C.

(*Read April 22, 1910.*)

On May 12, 1902, a series of accidents from fire and explosions occurred at or contiguous to the Sheraden Yard of the P. R. R. at Pittsburg, Pa., as the result of the collision of a tank car containing "stove naphtha" with another car, or cars, whereby the tank car was perforated and naphtha escaped. As the collision occurred in the late afternoon at a time when the yard switch lamps and the lamps of other safety devices had been lighted, the vapors of the naphtha which was spilled from the perforated tank car, became ignited and through the conflagration thus initiated other tank cars containing naphtha were heated to such a point as to force their contents out where they could contribute to the general conflagration, or, where, as in one instance certainly, the tank valve was too firmly set to thus yield to this effect, the tank itself was ruptured and its heated contents were discharged into the atmosphere, producing most disastrous results.

The Sheraden Yard was artificially constructed by filling a ravine between steep hills extending from the high ridge along the of level surface required for so extensive a railroad yard. As Ohio River, near the confluence of the Alleghany and Monongohela rivers, to the northwest of Pittsburg, so as to obtain the area might have been expected, the bottom of the ravine was occupied by a stream, known as Cork Run, and this was fed by several lateral streams which entered the ravine through fissures or ravines in the surrounding hills. Good engineering demanded the preservation of these water courses after the filling of the ravine, and this was done by means of sewers, the main one occupying, in general,

the bed of Cork Run, and its laterals those of the tributaries to Cork Run. The total length of the sewerage system thus constructed to the point where the collision occurred was 2,785 feet and the different sections varied in diameter from 2 feet at the head to 10 feet at the mouth where the sewer opened into the unfilled portion of the ravine near the Ohio River.

Immediately beside and parallel with the Ohio River was the road bed of the P. & L. E. R. R., a culvert having been built over Cork Run. Immediately beside this railroad embankment, but between it and the bluff was a turnpike road including its wooden bridge across Cork Run, this bridge being 19 feet above the bed of the run. Cork Run from the mouth of the sewer to the point of its discharge into the Ohio River, a distance of about 116 feet, was practically an open drain. The building of the tunpike across the ravine made a large pocket or basin at this point.

Naturally use was made of Cork Run sewer in draining Sheraden Yard, and, as a part of this system, catch basins for surface water from the yard were built and connected to the sewer, one of these catch basins being near the point at which the collision occurred.

Among other explosions, one occurred in the basin between the mouth of the sewer and the turnpike which was sufficiently violent to wreck many buildings in the vicinity and to lift from its track a trolley car, which was just approaching the turnpike bridge, with sufficient force to injure some of the passengers.

Among other theories, it was alleged that this explosion was due to naphtha which ran into the sewer through the catch basin above referred to, where its vapors formed, with air, a combustible mixture; that the vapors in the sewer were ignited by the fire burning in the yard about the mouth of the catch basin; and that the flame was transmitted through the sewer and caused the explosion of the accumulated air-vapor mixture in the basin.

In considering the probability of this theory being the correct one, the dimensions of the sewer throughout its complete length was ascertained and, from the volumes calculated from this data, it was found that 3,451 gallons of naphtha would furnish sufficient vapor to completely fill the sewer, while 200 gallons of the naphtha

would fill the sewer with a slightly combustible but non-explosive mixture. As the tank car, which was perforated beside the catch basin, contained 7,253 gallons of naphtha, there was a sufficient volume to more than satisfy the requirements for filling the sewer with naphtha vapors as set forth above.

That explosions occurred in or about portions of the sewer and its laterals seemed undoubted, yet recalling the safety lamp, the difficulty experienced in transmitting flames through tubes, and the varying capacities of columns of explosives of different diameters in propagating detonation, a doubt arose in my mind as to the flame traversing this sewer. Not finding any information on this matter in literature, experiments were made as follows:

RESULTS OF EXPERIMENTS.

No.	Date, 1903.	Kind of Tube.	Diameter, Inches.	Length, Inches.	Ratio of D : L.	Result.
1	Apr. 23	Glass	1.5	38	1 : 25.33	Positive
2	23	"	1.5	32	1 : 25.33	"
3	24	"	1	49	1 : 49	"
4	24	Steel	4	240	1 : 60	"
5	22	Glass	0.4	25⅜	1 : 63.44	Negative
6	22	"	0.4	25⅜	1 : 63.44	"
7	22	"	0.4	25⅜	1 : 63.44	"
8	23	"	0.4	25⅜	1 : 63.44	"
9	23	"	0.4	25⅜	1 : 63.44	"
10	23	"	0.4	25⅜	1 : 63.44	"
11	23	"	0.4	25⅜	1 : 63.44	"
12	24	"	1	78	1 : 78	"
13	24	"	1	78	1 : 78	"
14	24	"	1	78	1 : 78	"
15	24	Steel	4	480	1 : 120	"
16	24	"	4	480	1 : 120	"
17	24	"	4	480	1 : 120	"

It having been observed that if vessels were filled with the combustible mixture of naphtha vapor and air and the mixture was ignited at the mouth of the vessel, the flame retreated to various depths depending on the relation of the diameter of the vessel to its length, experiments were made with tubes. In conducting the experiments in each case the tube or pipe, which was open at both ends, was inclined at an angle and a considerable quantity of liquid naphtha was poured slowly into it at the upper end. So soon as the last of the liquid had been poured into the tube a flame was

applied to the upper end of the tube (called the mouth) to ignite the vapors and it was then noted if the flame traveled completely through the tube and issued from the bottom end or if it traveled but part way through and became extinguished. In every experiment there was some liquid naphtha as well as naphtha vapors throughout the tube at the moment when the flame was applied. Experiments were made in glass and steel tubes varying in diameter from .4 inch to 4 inches. The results are presented in the following table. When the flame traveled completely through the tube and issued from the bottom end the result is marked "positive." When the flame traveled but part way down the tube and became extinguished the result is marked "negative."

It appears then that where the length of the tube is 63.44 times its diameter or above this the flame does not travel through.

RESULTS OF RECENT RESEARCHES IN COSMICAL EVOLUTION.

(PLATE XXXI.)

BY T. J. J. SEE

(*Read April 23, 1910.*)

Early in 1908 the writer was able to conclude the researches on earthquakes, mountain formation, and kindred phenomena connected with the physics of the earth, which this society did him the honor to publish in four memoirs,[1] and it then became possible to resume the study of the problems of cosmogony, which have been before him uninterruptedly since 1884. For a long time these problems have defied the powers of the mathematician and natural philosopher, yet for the last thirty years enough progress seemed always to be in sight to stimulate the hope and energy of investigators; and many papers have appeared on the subject, especially from Professor Sir G. H. Darwin, while lesser contributions have been made by Lord Kelvin, Newcomb and Poincaré, and others.

The problem of the heat of the sun, due to condensation under gravitation, had been successfully attacked by Helmholtz as early as 1854, and subsequently much improved by the researches of Lane, Lord Kelvin, Ritter and the writer;[2] but the discoveries in molecular physics made during the past decade have shown that the conclusions based on the theory of gravitation alone have to be modified to take account of the energy of molecular transformation made familiar in radio-activity. By this hitherto unsuspected reservoir of natural forces the maintainance of the activity of the sun and stars is greatly prolonged. Accordingly, instead of reckoning the life of the solar system in twenties of millions of years, as estimated by Helmholtz, it is now known that the actual duration

[1] Cf. PROCEEDINGS, 1906–1908.
[2] Cf. *A. N.*, 4053.

of our system is to be estimated only in billions of years. This prolongation of the period allowed for the activity of the sun is confirmed by other investigations made by the writer during the past two years, showing that the mode of formation of our system has been excessively slow and very different from what has been generally supposed; and that the principal agency which has operated in its development and in shaping the orbits has been the action of a resisting medium extending over immense periods of time.

I shall not attempt to treat fully of this large subject in the present communication, because it is to be discussed in considerable detail in the second volume of my "Researches on the Evolution of the Stellar Systems," which is now in press and expected to appear during the coming summer or autumn. But it may be useful to give a brief review of the subject to see where we stand, and to sum up what appears to be established by the investigations which have so fully absorbed my energies during the past two years. The present discussion must therefore consist mainly of a summary, and such explanations as will render it moderately intelligible to the general reader.

In the first place, it is desirable to remark that, for nearly a century, we followed Laplace's assumptions in regarding the planets as having been detached or thrown off from the sun, and the satellites as having been likewise thrown off from their several planets; and our problem was to find out how the postulated rings of vapor had condensed into the bodies now observed in the solar system. Thus with a fixed premise we were trying to find out how the planetary development had come about, and it scarcely occurred to any one that the premise itself might be false, and therefore all the efforts based on it in vain.

The turning point which enabled me to discover this error in the premises was a certain criterion based on the law of areas proposed by the distinguished French physicist Babinet, in 1861.[3] In this brief communication of three pages to the Paris Academy of Sciences, Babinet pointed out the contradiction of Laplace's cos-

[3] Cf. *Comptes Rendus*, March 18, 1861.

mogony resulting from the dynamical principle of the conservation of areas, and concluded that the classic nebular hypothesis was vitiated by a fallacy.

Like most negative critics, Babinet was beset by the weakness that he could tear down but could not build up. And as he substituted nothing for the theory of Laplace, the valuable criticism which he had made was scarcely noted by his contemporaries, and remained practically unknown to subsequent investigators. Thus we find in the writings of Lord Kelvin, Newcomb, Darwin, Tisserand and Poincaré no mention of the eminently useful criterion which Babinet had proposed in 1861; and it was allowed to slumber half a century in a forgotten number of the *Comptes Rendus.*

Looking back over this strange state of affairs, we naturally ask ourselves why Babinet did not proceed further with his investigations. To this question no certain answer can be returned, but it is probable that his inability to account for the remarkable roundness of the planetary orbits, which Laplace had explained by the process of gentle detachment, made him hesitate to go any further, and he gave up the effort in despair.

More than forty years ago the American astronomers Kirkwood and Pierce reached the conclusion that a mass so rare as the solar nebula must have been when it was expanded to fill the orbits of the planets, as imagined by Laplace, could not exert hydrostatic pressure so as to detach rings or zones of vapor; and they urged this objection as essentially fatal to the Laplacian theory. Here again the criticism was of the negative kind, like most of the criticism of Laplace's theory which appeared before and since their epoch; and it therefore shared the inherent weakness of all criticism which is not accompanied by work of constructive character. It does little good to break up our mental images if we cannot put better ones in their places; tearing down is easy, but building new structures much more difficult. And so long as the criticism was of purely negative character, it naturally failed to supersede Laplace's theory with a better one, and, by default, the ring theory has continued to hold its ground almost up to the present time.

It is not necessary to go into the details of more recent destruc-

tive criticism, except to say that no absolutely conclusive results were obtained until about two years ago, when I recognized from Babinet's criterion that we must unconditionally abandon the historical view that the planets have been detached from the sun and the satellites detached from their several planets. This led to the capture theory, which gives us the true conception of the origin of the solar system, and of other cosmical systems existing in space.

The earliest suggestion of the capture theory runs back a good many years, but it has not heretofore been accepted by professional astronomers familiar with celestial mechanics, because there was no recognized way in which the very circular orbits could be accounted for, except by the detachment theory of Laplace. It is only since the writer's discovery of the paramount part played by the resisting medium, some two years ago, that the capture theory has taken a form consistent with the established principles of dynamics.

As throwing light upon the earliest notice of the capture theory, we may quote a passage from Professor Barnard's article on "Jupiter's Fifth Satellite" in *Popular Astronomy*, for October, 1893, which I came across in making up the engravings for Volume II. of my "Researches" now in press. Barnard says:

> As was the case when Professor Hall discovered the satellites of Mars, many theories have been offered to account for the presence of the new body (Jupiter's fifth satellite). The asteroid zone between Mars and Jupiter is an endless source of material for such theories. It was suggested in 1877 that the Martian satellites were asteroids captured by Mars from the asteroid zone lying outside his orbit. These same theorizers have not failed to come up again and suggest that the fifth moon of Jupiter is a captured asteroid from the zone of asteroids lying inside the orbit of Jupiter. They never try to account for satellites of Saturn, Uranus and Neptune this way, because the asteroid mine is too far off; yet they are similar bodies, and undoubtedly had a similar origin. There is no question that this satellite has been there all along, and for infinite ages has been performing its revolutions about the planet, undetected until the night of 1892, September 9.

This account by Professor Barnard is exceedingly clear, and of great historical interest. It confirms the statement often made that the idea of capture was first entertained by amateurs. The doctrine has grown, however, from the theory of the capture of comets, which has been a subject of investigation by the most emi-

nent professional astronomers since the celebrated work of Laplace and Burkhardt, about 1805, on Lexell's comet of 1770. It is remarkable that in the course of the past century the capture theory of comets should have been highly developed by Leverrier, Adams, Schiaparelli, H. A. Newton, Callandreau, Tisserand and many others, while the capture of satellites should not have been seriously considered prior to the publication of the writer's "Dynamical Theory of the Capture of Satellites," in the *Astronomische Nachrichten*, Nos. 4341–42 (July, 1909).

The field of research opened up by the new theory is so large that it doubtless will be many years before it is exhausted; yet enough seems to have been done already to assure us that all the satellites are captured bodies. The result of this line of thought will be practically a new cosmogony. In order to render the results intelligible, we shall first outline the process of capture, and then recapitulate the conclusions at which we have arrived.[4]

(α) In the year 1836 the celebrated German mathematician Jacobi communicated to the Paris Academy of Sciences an integral of the differential equations of motion for the restricted problem of three bodies; the system being made up of a sun attended by a planet revolving about it in a circle, and a particle of insensible mass. Jacobi remarks that the integral may be applied to a body such as the terrestrial moon.

(β) In 1877 Dr. G. W. Hill developed and greatly perfected the theory of Jacobi's integral, and applied it to the lunar theory in a series of celebrated papers. Hill's work has since been the basis of the profound researches of Poincaré, Darwin and others on "Periodic Orbits" and related topics in celestial mechanics.

(γ) Dr. Hill showed that in the restricted problem of three bodies, implied in Jacobi's integral, there is a partition of the whole space into three parts—one about each of the large bodies, the sun and planet, and a larger domain enclosing both bodies—within which the power of control over the particle is vested in the two bodies individually and collectively, respectively. The closed surface about the earth includes the orbit of the moon, and the orbits of the other

[4] Cf. *A. N.*, 4341–42, 4343, and Publications A. S. P., No. 127, August, 1909.

satellites in like manner are within the closed surfaces about their several planets; and Dr. Hill remarks that this arrangement is necessary to secure stability. If a satellite is once within this region, with the surface of zero velocity closed about it, it cannot escape, but will always remain attached to the planet, and its radius vector will have a superior limit. How the moon and other satellites came within these closed regions Dr. Hill did not inquire; and subsequent investigators appear to have supposed that as these bodies cannot now escape from their planets, so also they cannot have come in from a remote distance, but must have originated where they now are. This is the view put forth by Moulton in his discussion of Professor W. H. Pickering's suggestion that Phœbe had been captured by Saturn; but such reasoning is easily shown to be erroneous by the following considerations:

(δ) Jacobi's integral, as originally given by him, is based on the differential equations for unrestricted motion in *empty space*, and no account is taken of the additional terms which must be added to the differential equations of the motion of the sun, planet and particle, when the motion is very slightly conditioned by the introduction of a nebular resisting medium, such as existed in the early history of our system, and is now observed to be widely diffused throughout nature. Jacobi's original integral, therefore, requires the addition of a secular term to represent the actual movement of a sun, planet and particle; and the complete expression for any particle whose coördinates are x_i, y_i, z_i becomes

$$x_i^2 + y_i^2 + \frac{2(1-\mu)}{\sqrt{(x_i - x_1)^2 + y_i^2 + z_i^2}} + \frac{2\mu}{\sqrt{(x_i - x_2)^2 + y_i^2 + z_i^2}} = C_i + a_i t_i. \qquad (1)$$

The secular term $a_i t_i$ makes the constant C_i increase with the time.

(ε) Now the surfaces of zero relative velocity, which define the closed spaces about the planets, have larger values of C the nearer we approach to the sun or planet. This is easily seen in the accompanying plate from Darwin's celebrated memoir on "Periodic Orbits." When the particle or satellite revolves against resistance, therefore, the second member of (1) increases, and there is a secu-

lar shrinkage of the surface of zero relative velocity. *Accordingly the particle drops down nearer and nearer these centers, and the surface finally becomes closed, leaving it no longer free to move about both bodies in the hour-glass shaped space, as formerly, but*

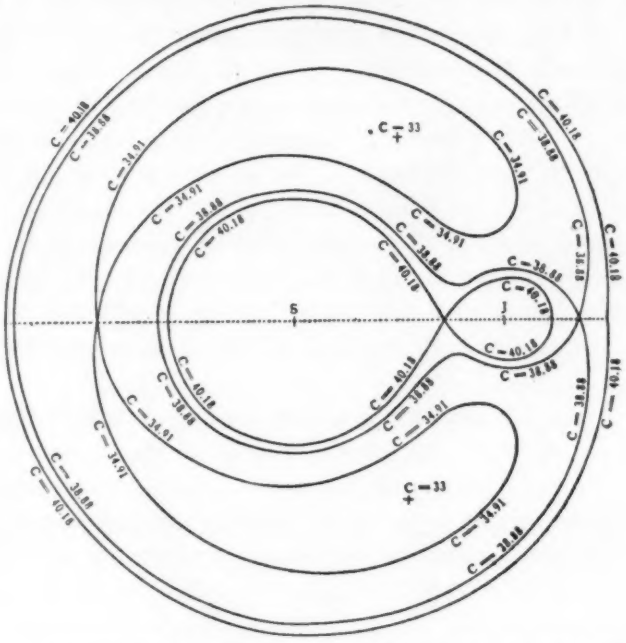


FIG. 1. Curves of Zero Velocity (Darwin). This diagram illustrates the hour-glass shaped space through which the particle may move and drop down nearer the sun or planet, till it becomes captured by one of the larger bodies.

restricted to the sphere of influence controlled by the sun or planet individually, as the case may be. The particle which once revolved about both the sun and planet can no longer do so, but becomes an inferior planet (satellite of the sun) or a satellite of the planet.

It is shown that the mean distance of the planet decreases as well as that of the satellite, but that the action of the resisting medium is relatively more effective on the satellite than on the planet in the inverse ratio of their radii, and therefore the planet approaches the sun very slowly and the satellite very rapidly. Accord-

ingly we may generally ignore the effect on the planet, and consider only the effect on the satellite. It is thus clear how the satellite drops down near these centers of attraction, and is finally captured by one of them.

(ζ) This is how all the satellites of the solar system were captured. At first they moved principally under the attraction of the sun, and could pass from the sun's to the planet's domain, through the neck of the hour-glass shaped space connecting the two spheres of influence. When the neck is narrow, Darwin says that a particle which passes from the sun's to the planet's control may revolve about it hundreds of times before quitting the planet's sphere to return again to the sun's control. And if resistance is meanwhile encountered, so that the neck of the surface of zero velocity becomes closed, it is clear that the particle never will quit the sphere of the planet's control, but will abide there permanently as a satellite.

(η) Thus it incontestably follows that the satellites of Jupiter, Saturn and other planets formerly moved about the sun, and since they were captured have had their orbits reduced in size and rounded up under the secular action of the resisting medium formerly pervading our solar system. *Satellites may cross over the line SJ before coming completely under the planet's control, in which case they will move retrograde.* In such cases the neck connecting the two spaces is extremely narrow. But as the neck usually is not so narrow as to produce crossing satellites, most of them naturally move direct, in accordance with observation. *This is the reason also why the planets have direct rotations on their axes. The planets have in no case been inverted, as some have recently supposed, in order to account for the retrograde motions of the outer satellites of Jupiter and Saturn.*

(θ) In the case of the terrestrial moon it is shown that the earth simply captured one of the twenty-seven million such planets which went to form the sun's immense mass. The moon came to us from the depths of space, and never was a part of the earth, as has long been supposed. Professor Sir G. H. Darwin's celebrated work of 1879 is shown to be based on chance coincidences, and not actual

physical history. All the details of the lunar terrestrial system are known to accord with the theory that the moon is a captured planet. (*a*) *It is shown by rigorous calculation from the theory of probability that the chances are infinity to one that the moon was captured like the other satellites.* (*b*) *It is likewise shown that the probability is infinity to one that the earth could not have rotated with sufficient rapidity to detach the moon.* As the theoretical possibility of the capture of the moon is beyond doubt, it is therefore certain that it actually occurred.

With this brief outline of the process of capture, we shall now sum up the conclusions at which we have arrived.

1. The planets never were detached from the sun by acceleration of rotation, as held by Laplace, but had their origin in the outskirts of the solar nebula, and have since neared the sun and had their orbits reduced in size and rounded up into almost perfect circles by moving for long ages against the resisting medium of nebulosity formerly pervading our system.

2. The view that the planets were formed at a great distance from the sun was advanced by the great Swiss mathematician Euler in 1749, before the theories of Kant (1755) and Laplace (1796) were promulgated; and he, too, based his conclusion on the secular effects of a resisting medium. Euler considered only the effect on the mean distance, but in 1805 Laplace showed by a more general investigation that the eccentricity also would be diminished; and it is this latter effect in producing the observed roundness of the orbits that gives us the principal clue to the true mode of formation of the solar system.

3. It therefore follows that the planets were developed in the solar nebula, but are in no sense of the word children of the sun; for they never were connected with the sun by any form of hydrostatic pressure and afterwards thrown off by acceleration of rotation, as has been generally believed.

(4) The satellites likewise never were mechanically connected with their several planets through any form of pressure and fluid equilibrium, but were originally independent planets, moving in regular elliptical orbits, and were afterwards captured and attached to

the planets about which they now revolve as satellites. Thus the satellites are in no sense of the word children of the planets and grandchildren of the sun, as we have been so long taught; but were formed in outer parts of the solar nebula, and simply survive out of a much vaster number of small bodies which have been swallowed up in laying the foundations of the sun and principal planets.

5. The best surviving illustration of the primordial condition of our system is afforded by the asteroids. The system was once quite filled with these satellites, and they revolved in orbits which were so eccentric that they crossed the paths of several of the planets. In time they have been largely absorbed in the sun and planets, and what remain have been thrown within the orbit of Jupiter, where they now revolve in comparative stability.

6. That our moon, likewise, was originally a planet which neared the earth and was finally captured and made a satellite. It was no part of the terrestrial globe detached by rapid rotation, as has been generally believed since the time of Anaxagoras, B. C. 500–428, and more recently taught by Laplace, Lord Kelvin, Sir George Darwin, Poincaré and other eminent writers.

7. The theories long current in geology and astronomy that the earth once rotated in two or three hours, so rapidly that it threw off a layer of peripheral matter now collected into the moon, are shown to be quite devoid of foundation. The effect of this advance will be to show that the globe never was highly oblate, and to correct the theories of geology, and greatly improve the science of astronomy.

8. The rotations of the planets on their axes have been produced by the capture and absorption of satellites. It is shown in the theory of the restricted problem of three bodies that most of the satellites should move direct, and it is by the capture and absorption of millions of these small masses that the planets have been given direct rotations on their axes.

9. The modification of the original obliquities of the planets is shown to depend on the capture and absorption of satellites. It is shown that in this way the obliquity of Jupiter was practically obliterated. If Saturn's mass were to be augmented till it became equal to that of Jupiter, his obliquity likewise would disappear.[5]

[5] Cf. *A. N.*, 4367.

The work of Stratton, implying planetary inversion, is shown to be inapplicable to the solar system, because based on a false premise.

10. As the satellites have all been captured, but were once independent planets, it will not seem strange that a few of them have retrograde motions; the capture theory explains this in a simple and easy manner, and it is in accord with the latest researches on the celebrated problem of three bodies, the treatment of which has been much improved by Jacobi, Hill, Poincaré, Darwin and other mathematicians.

11. As the planetary rotations are due to the capture and absorption of satellites, theory shows that the larger planets, as Jupiter and Saturn, ought to rotate most rapidly. This is in accordance with observation, and the nature of the cause at work shows that the earth never could have rotated much if any more rapidly than at present.

12. The cause of the secular acceleration of the moon's mean motion has been one of the leading problems of astronomy for over two centuries. In spite of all the researches of the greatest mathematicians, there remains an outstanding inequality of about $2''$ which cannot be accounted for by gravitational or other definite theory. This anomaly is now explained by the fact that the moon is a captured planet, and therefore slowly nearing the earth, owing to the action of a resisting medium, in the nature of cosmical dust pervading the regions where the planets move.

13. The roundness of the orbits of the planets and satellites has been remarked from the earliest ages of science, and this phenomenon, which led Plato, Aristotle and other Greek sages to declare that the heavenly motions are perfect because they are circular, is now shown to be due to the secular action of a resisting medium which has reduced the size of the planetary orbits and well-nigh obliterated their eccentricities; and not at all to these bodies having been detached by rotation and set revolving in orbits which were originally nearly circular, as incorrectly held by Laplace.

14. Around each planet there circulates a vortex of cosmical dust, of which the satellites alone are large enough and bright enough to be visible in our telescopes. The descent of this material

against the surfaces of the sun and planets gives rise to the observed but heretofore perplexing accelerations of the equatorial regions of these globes.

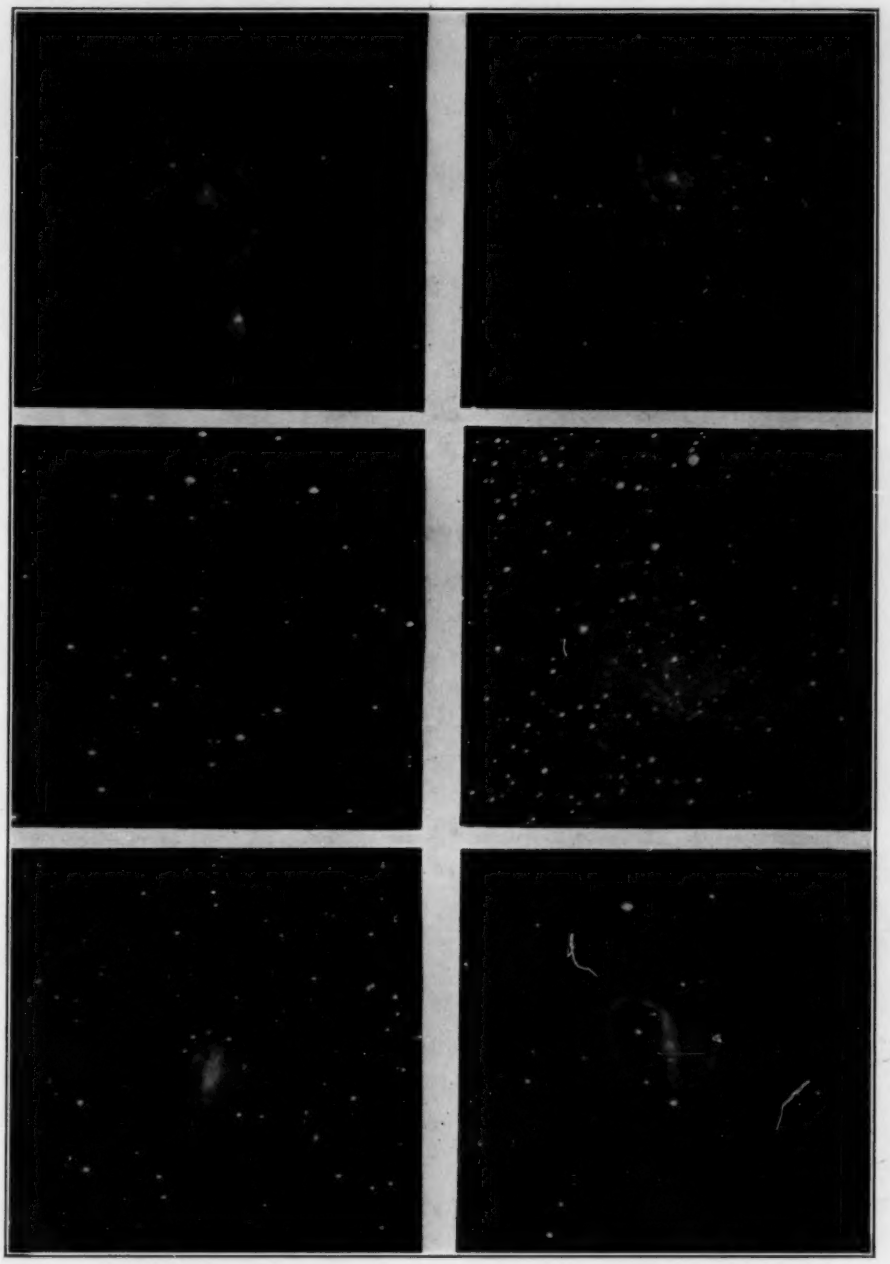
15. The descent of matter upon the sun increases its mass and gives rise to a small secular acceleration of the earth amounting to about 0″.75 per century, which, as Mr. Cowell has shown, is also indicated by the observations of the eclipses during the past 3,000 years.

16. And just as the earth never rotated very rapidly, and has not been appreciably retarded by the effects of tidal friction, so also Venus likewise has escaped a corresponding retardation of axial rotation, and still rotates in 23 hours 21 minutes, as has been held by observers since the days of Cassini, 1667. Accordingly it follows that the conditions of this planet are more like those of the earth than any other body of our system. Mars rotates 41 minutes slower than the earth, while Venus rotates 35 minutes faster; and as the former planet is about as much outside of the earth's orbit as the latter is inside, there is seen to be a profound physical cause which has operated to establish the period of 23 hours 21 minutes first inferred from observations taken over two centuries ago. The planet Venus, therefore, is habitable and probably inhabited by some kind of intelligent beings.

17. The moon was once an independent planet, and when revolving in the region of the asteroids, suffered numerous collisions with satellites; this is the cause of the immense, round, sunken craters which have puzzled astronomers since the time of Galileo. The traditional theory that they are volcanic is quite devoid of foundation, and now definitely and finally disproved.

18. The solar system extends much beyond Neptune, and several of the unseen planets revolving near the outskirts of the system may yet be discovered by observation. Neptune's orbit is too round to be the last of the planets, because this roundness indicates that the nebulous medium was quite dense at that distance, and consequently that the limits of the system are much further out.

19. It has long been known that the periodic comets are captured bodies which have suffered transformation of their orbits



Spiral Nebulae Photographed at Lick Observatory by Keeler and Perrine (Publications Lick Observatory, vol. VIII).

M. 51 Canum Venaticorum; M. 101 Ursae Majoris;
H. IV 13, Cygni; H. IV 76, Cephei;
H. I 53, Pegasi; H. I 55, Pegasi.

These photographs show that the attendant bodies are not thrown off by rotation, but begin forming in the distance and are gathered in towards the center as the whirlpool settles under its own gravitation.

under the action of the planets. The theory is now extended to the asteroids and satellites, and these two classes of bodies are shown to be closely related. The survival of satellites near the principal planets shows that our system was once filled with these small masses. Such moons naturally developed in the condensation of a nebula, and all nebulæ include multitudes of solid globes in addition to the gaseous matter shown by the spectroscope.

20. The collisions of satellites with the larger globes, as shown by the battered surface of our moon, give rise to part of the light of the nebulæ, and no doubt also to some of the cosmical dust with which they are filled.

21. The whirling of a spiral nebula is due to the unsymmetrical meeting of two streams of nebulosity, which thus coil up, and settle down under the effects of mutual gravitation; or to the mere gravitational settling of a nebula of unsymmetrical figure. The inevitable result is a whirling cosmical vortex, and eventually a star surrounded by a cosmical system.

22. Nearly all single stars have planetary system revolving about them, and in the immensity of the starry heavens an infinite number of the planets are habitable, and no doubt actually inhabited. Life is almost as general a phenomenon in the universe as matter itself, though our dominant materialistic philosophy is loth to admit it.

23. We can see only visual and spectroscopic binary stars in the sidereal universe, owing to the immense distances which render the smaller bodies wholly invisible in our telescopes; but we know, from the example of the solar system and from the causes which have operated in its formation, that planets and satellites exist everywhere about the fixed stars.

24. In star clusters the motion is shown to be of a spiral character, as in the nebulæ, where we can trace the streams of movement by the nebulosity, and the same theory is applied to the milky way. It is shown by calculation based on the best modern data, that the extent of the sidereal universe actually exceeds the vast dimensions found by Sir William Herschel, and of late years held to be extreme.

It will readily be understood that the lines of argument by

which these results are established are of mathematical character, and in accordance with the principles of dynamics. It is shown, for example, that the spirals observed among the nebulæ are *chance spirals,* and not true geometrical figures. And the general theory is established that the nebulæ are formed by the falling together of cosmical dust expelled from the stars by the radiation-pressure of their light and by electric forces. When this fine dust is precipitated under the action of cathode rays it forms meteorites, and the collection of meteorites forms satellites and larger cosmical bodies.

A nebula is a cloud of cosmical dust with moons and planets intermixed. The falling together of such nebulosity necessarily produces cosmical vortices, and these are the whirlpool nebulæ so long observed in the heavens but not heretofore understood. The expulsion of fine dust from the more active stars gives the primordial material for the formation of nebulæ; the condensation of the nebulæ in turn forms stars, so that the total process is a cyclical one, involving both the aggregation of matter into stellar centers and its subsequent diffusion to form new nebulæ, stars and systems.

As the second volume of my "Researches on the Evolution of the Stellar Systems" is a work of nearly 750 pages, it will readily be understood that this summary is too brief to give more than a faint outline of the work. Yet as the results are of general interest to a large class of readers, I have thought they might be mentioned in this brief review.

Our age is a peculiar one, in that with the progress of astronomy vast masses of observational data are accumulated by the persevering industry of self-denying men of science; but so long as these data cannot be put together to yield us the long-sought laws of cosmical evolution, the outcome is almost as vain as the weaving of Penelope's web. Natural philosophers believe, however, that the time is now auspicious for a great advance, not merely in the details, but also in the laws and principles of exact astronomical science. One of the ultimate aims of the physical sciences in all ages has been the discovery of the laws of cosmical evolution.

If with the modern improvements in the mathematical treatment of the problem of three bodies, and the observational data derived from photographic study of the nebulæ and clusters, as well as from the visual and spectroscopic binary stars, this progress be not possible in our time, it is difficult to see how better results can be expected in the future.

I have therefore labored with no ordinary energy to weave together the scattered and discordant threads of argument regarding phenomena which heretofore could not be brought into harmonious relationship. And I have been fortunate enough to attain an unexpected degree of success; so that I cannot doubt that the result will go far towards a permanent solution of the problems of cosmical evolution, which is certainly an urgent *desideratum* of our time.

U. S. Naval Observatory,
Mare Island, California,
April 8, 1910.

THE EXISTENCE OF PLANETS ABOUT THE FIXED STARS.

By T. J. J. SEE.

(*Read April 23, 1910.*)

The question of the existence of planets about the fixed stars is an old one, and has been more or less discussed by astronomers ever since the popularization of Copernican doctrines by Giordano Bruno, who suffered martyrdom at Rome in the year 1600. Up to the present time, however, there has been no rigorous criterion for the construction of a conclusive argument; and the discussion has been comparatively unprofitable, except in the development and expression of free opinion. Disputations leading to the expression of individual opinion may be of some value, because new ideas may thus be suggested, and accordingly such habits have been encouraged since the days of the Greeks, as we learn from the collections of opinions handed down by such writers as Diogenes Laertius.

But to render such efforts effective from a scientific standpoint it is necessary to find criteria which make it possible to build up a conclusive argument. The discussion then ceases to be a mere record of individual opinion, and becomes an integral part of science supported by the necessary and sufficient conditions required to ensure the validity of accurate mathematical reasoning. This improvement in our knowledge of the existence of planets about the fixed stars has been made possible by the writer's recent discoveries in cosmical evolution, and we shall, therefore, give a brief summary of the argument as it stands today.

So long as we did not know the exact process involved in the formation of the solar system it was possible to argue that just as planets exist about our sun, so too, they may by analogy be inferred to exist also about other fixed stars. This natural inference rests on the implied uniformity of the creative process involved in the development of the planets. Obviously we could not observe

planets at the great distance of the fixed stars, while the double and multiple stars constitute systems of very different character. There was, therefore, no direct observational evidence that planetary formation was a part of the usual order of nature. The process by which our solar system arose was involved in great doubt and obscurity, and could not be definitely made out, notwithstanding the labors of many eminent mathematicians during the past century. No longer ago than 1906 the late Professor Newcomb declared[1] that he still retained "a little incredulity as to our power in the present state of science to reach even a high degree of probability in cosmogony."

I have recently shown that the principal difficulty in all the efforts of mathematicians for solving the problems of cosmogony has arisen from false premises which had come down from the days of Laplace, and thus vitiated all our reasoning. It had been uniformly assumed that the planets were thrown off from the sun, and that this process of detachment by rotation of the central mass had set them revolving in orbits of small eccentricity. Laplace's postulates in some form or other had been assumed by all investigators since 1796. And it is curious to notice that Laplace in turn had merely extended the conceptions developed by Newton in his treatment of the problem of the figure of equilibrium of a rotating mass of fluid.

For, in establishing the theory of universal gravitation, in 1686, Newton had correctly explained the figures of the earth and other planets as due to the effect of gravitational attraction combined with the centrifugal force due to axial rotation, thus giving various degrees of oblateness depending on the intensity of the forces, and the heterogeneity of the planetary masses. These results followed from the theory of gravitation, and Newton had applied to them the same masterly reasoning which he usually exhibited in the treatment of mathematical problems.

Not long after the epoch of Newton the problem of the determination of the figures of equilibrium of rotating masses of fluid was considerably improved by the researches of Maclaurin, while subsequently Laplace himself extended and confirmed the results

[1] In *Popular Astronomy* for November, 1906, p. 572.

of his predecessors. Though they did not deal with definite cases of extreme oblateness, the great mathematicians of the eighteenth century made it clear that very rapid rotation would be adequate to produce disc-shaped figures of equilibrium.

This subject is quite fully discussed by Laplace in the "Mécanique Céleste,"[2] where the following theorems are announced.

> Any homogeneous fluid mass of a density equal to the mean density of the earth, cannot be in equilibrium with an elliptical figure, if the time of its rotation be less than 0.10090 day. If this time be greater, there will always be two elliptical figures, and no more, which will satisfy the equilibrium. If the density of the fluid mass be different from that of the earth, we shall have the time of rotation, in which the equilibrium ceases to be possible, with an elliptical figure, by multiplying 0.10090 day by the square root of the ratio of the mean density of the earth to that of the fluid mass. Therefore with a fluid mass whose density is a quarter part of that of the earth, which is nearly the case with the sun, this time would be 0.20180 day, and if the earth were supposed to be fluid and homogeneous, with a density equal to a ninety-eighth part of its present value, the figure it must take to satisfy its present rotatory motion, would be the limit of all the elliptical figures, with which the equilibrium could subsist.

What Laplace here points out was in fact established by Maclaurin in his "Treatise on Fluxions," Edinburgh, 1742. For if k^2 be the gravitational constant, the density of the mass and ω the angular velocity of rotation, then it was proved that for

$$\frac{\omega^2}{2\pi k^2 \sigma} = 0.22467$$

the two possible figures of equilibrium coalesce into one, but for

$$\frac{\omega^2}{2\pi k^2 \sigma} > 0.22467,$$

there is no ellipsoid of revolution which is a figure of equilibrium. For very small values of $\omega^2/2\pi k^2\sigma$, there are two distinct ellipsoids which are figures of equilibrium, one of them being nearly spherical and the other very oblate, the limits, for $\omega = 0$, being respectively a sphere and an infinite plane.[3]

[2] Liv. III., Chap III., § 20.

[3] Tisserand's "Mécanique Céleste," Tome II., chap. VI.

If a very flat disc could exist as a figure of equilibrium, it was natural to imagine that such figures might have had a part in starting the planets in their round orbits. The great roundness of the orbits of the major planets and of the satellites then known, and their uniform direction of motion near a common plane suggested to Laplace that these orbits had once been nearly circular, and that the bodies had developed from rings like those of Saturn. It appeared to him that they had resulted from the condensation of rings of vapor gently detached from the equators of the bodies which now govern their motions. This reasoning of Laplace was logical, and necessarily resulted from the researches of Newton on the figures of equilibrium of rotating masses of fluid, and the subsequent extension of these researches by Maclaurin, D'Alembert and other mathematicians; and the procedure seemed so plausible that its correctness was assumed by all subsequent investigators.

Thus Lord Kelvin, Newcomb, Darwin, Tisserand, Poincaré and others accepted the principles of Laplace as laid down in his formulation of the nebular hypothesis, and proceeded to work out the details of planetary development. It is true that Kirkwood, Pierce and others had made objections to the Laplacian hypothesis, based on the inability of a medium so rare as the postulated nebula to transmit hydrostatic pressure from the center outwards, but such destructive criticism was of little avail so long as the roundness of the orbits could be explained only by Laplace's hypothesis of detachment. The persistence of Laplace's classic nebular hypothesis, in spite of negative criticism, was therefore inevitable. But as the greatest mathematicians were unable to make out the process of planetary formation, on the detachment theory, the whole subject remained one of contradiction and obscurity. In his address to the British Association at Capetown, in 1905, Professor Sir G. H. Darwin said:

> The telescope seems to confirm the general correctness of Laplace's hypothesis. . . . Nevertheless it is hardly too much to say that every stage in the supposed process presents to us some difficulty or impossibility. Thus we ask whether a mass of gas of almost inconceivable tenuity can really rotate all in one piece, and whether it is not more probable that there would be a central whirlpool surrounded by more slowly moving parts. Again, is there any sufficient reason to suppose that a series of intermittent efforts would

PROC. AMER. PHIL. SOC., XLIX, 195 O, PRINTED JULY 29, 1910.

lead to the detachment of distinct rings, and is not a continuous outflow of gas from the equator more probable? (p. 19).

So the matter stood until early in 1908, when the writer took up study of the spiral nebulæ and discovered that the roundness of the orbits of the planets and satellites was due to the secular action of a resisting medium, and not to detachment in orbits which were originally nearly circular, as imagined by Laplace.[4] The novelty of this view of the formation of the solar system is pointed out in a review by a distinguished authority in *Nature* of July 29, 1909, where we read:

> Dr. See contends that the planets and satellites of the solar system were captured and their orbits made remarkably circular by a resisting medium. In his view, therefore, Laplace's nebular hypothesis is altogether wrong, whereas the current view is that it is in the main right, though in need of considerable modification and extension. . . . Capture is a possibility, but Dr. See has done nothing to raise his theory beyond a mere conjecture, even though he points out, in addition, that a resisting medium would diminish the mean distance and the eccentricity of an elliptic orbit, and that in the case of Jupiter's satellites the outer orbits are highly eccentric and the inner orbits nearly circular.

This review was written before the papers on the dynamical theory of the capture of satellites,[5] and the capture of the moon[6] had appeared; so that the claim that I have not explained how capture takes place is no longer valid. On the contrary, this work has now been published nearly a year, and the correctness of it does not seem to be questioned in any quarter.

We shall not in this paper dwell on the work done by the author during the past two years, and embodied in Volume II. of the "Researches on the Evolution of the Stellar Systems," which is soon to appear, but shall merely remark that the Laplacian theory now appears to be permanently overthrown and the capture theory established in its place. It is proved that the embryo planets were formed in the outer parts of the solar nebula, and have been captured and attached to the sun after nearing it from a great distance, while the satellites have been likewise captured by their several planets.

[4] Cf. *A. N.*, 4308.
[5] *A. N.*, 4341–2.
[6] *A. N.*, 4343.

This proof that the planets never were any part of the sun, but have come to it from great distance, is accompanied by an argument based on dynamical principles showing that the same thing will happen for any other star developing in a nebula; and it is, therefore, certain that the other stars have planetary systems revolving about them. The argument is based on the recognized laws of dynamics and verified by the known history of the solar system, and therefore seems to be entirely satisfactory.

The difficulty and uncertainty, as to the existence of planets about the fixed stars generally, appears, therefore, to be overcome; and we conclude that the discovery of the true process of formation of our system enables us to affirm with confidence that nearly all the fixed stars have systems of planets revolving about them. Here is the foundation of the new line of argument.

1. The observed motions of the double stars show that the law of gravitation is universal, and that the same law of attraction that holds true for the bodies revolving about the sun also regulates the motions of the remotest stars.

2. This indicates that the forces are central, and that the same laws of areas and of motion hold there as in the solar system. Similar effects imply similar causes, and hence the double stars have been set revolving by projective forces and other causes analogous to those which set the planets in motion about the sun.

3. These movements resulted from nebular vortices, formed by the settlement and winding up of streams of nebulosity which did not pass through the center, but circled about it.

4. Such is the phenomenon shown in the spiral nebulæ, which are proved to exhibit the usual process in the development of cosmical systems. Nebulæ are formed from cosmical dust expelled from the stars, and it cannot fall to a center to produce a central sun without giving rise to a whirling vortex about that center, since but few of the streams will converge in a point.

5. Planets form in the streams which make up a spiral nebula, and in some cases they unite to form a double star, the system thus developing into a double sun; but in the more typical case the planets are too small to be seen and the stars appear to be single,

whereas in reality they are surrounded by planetary system not unlike that which revolves about our sun.

6. The spiral nebulæ indicate very plainly that the motion of the nebulosity is towards the center; that it was originally at greater distance, but at length captured and brought in nearer and nearer the center by the action of universal gravitation.

7. It is, therefore, plain that just as our planets were formed in our nebula at a great distance from the sun and afterwards had their orbits reduced in size and rounded up by moving against a resisting medium; so also planets revolving and passing gradually into order and stability are developed in the nebulous streams which by condensation have formed the other stars, and this makes possible the formation of planetary systems among the stars generally.

8. We may, therefore, feel entirely certain that the stars which appear to be single—about four fifths of all the stars—have planets revolving about them. And the other fifth are spectroscopic and visual binaries, the planets in this case being so large as to be visible in our telescopes or producing a relative motion in the line of sight which may be recognized by means of the spectrograph.

9. The historical difficulty of determining whether there are planets about the fixed stars may, therefore, be definitely overcome by the recognition of the true mode of formation of the planetary system. It is not exceptional, as was formerly believed. So long as we held that the planets were thrown off, it was not at all certain that the mechanical conditions would permit such detachments elsewhere in the universe, and our solar system might be held to be nearly if not quite unique. Now, however, all such views become inadmissible, and we see that our system is typical of the general order of nature.

10. Accordingly, although the planets about the fixed stars probably will always be invisible, and too small to be detected by the spectrograph, yet it is possible to be quite sure of their existence from the operation of known mechanical laws; and from the demonstrated mode of formation of the solar system. This is a practical application of the capture theory to the larger problems of the universe, and the result is of general interest to every

student of nature. The "Astronomy of the Invisible" thus takes on vastly greater importance than in the time of Laplace and Bessel. And we see that the sagacious suggestions made by these great astronomers nearly a century ago were well founded. And just as there are invisible planets about the luminous fixed stars, so also there probably are countless bodies everywhere in space which are essentially non-luminous. The amount of dark matter in the universe therefore is much greater than has been generally supposed, or heretofore considered probable.

U. S. Naval Observatory,
Mare Island, California,
March 28, 1910.

ON THE DISTANCES OF RED STARS.

(ABSTRACT.)

BY HENRY NORRIS RUSSELL.

(*Read April 23, 1910.*)

Comparison of the parallaxes of stars, derived by the writer from photographs taken at the Cambridge Observatory (England) by Mr. A. R. Hinks and himself, and their spectra, determined at Harvard under the direction of Professor Pickering, shows a marked correlation between spectral type and parallax.

If the stars observed are divided into four groups: (1) Those chosen at random for comparison purposes in fields near the Milky Way, (2) similar stars remote from the Galaxy, (3) stars of considerable proper motion, (4) those shown by observation to be relatively very near us (parallaxes greater than 0″.10); then the percentages of stars of the different spectral types in these groups are as follows:

Group.	Color Spectrum.	White.		Yellow,	Orange,	Red,
		A	F	G	K	M
1		30%	25%	19%	26%	
2		6	19	42	29	4%
3				50	38	12
4				10	60	30

The percentage of orange and red stars increases steadily with increasing nearness of the group to our system.

Conversely, if the observed parallaxes of the stars of considerable proper motion are compared with the predictions of Kapteyn's formula, it is found that the means for groups including all spectral types are closely represented, but that the means by spectral types show marked systematic deviations, as follows:

Spectrum.	Number of Stars.	Mean Parallax.		Ratio.
		Observed.	Formula.	
F8	3	0″.042	0″.079	0.5
G	7	0″.029	0″.104	0.3
G5	2	0″.041	0″.098	0.4
K	4	0″.119	0″.103	1.2
K5	5	0″.253	0″.141	1.8
M	3	0″.220	0″.108	2.0

As all the stars under consideration lie within the same limits of apparent brightness, it follows that they are intrinsically fainter the redder they are. The reddest average only one fiftieth as bright as the sun.

On the other hand, it is well known that many bright red stars, such as Arcturus and Antares, are at great distances, and are probably at least one hundred times as bright as the sun.

All this can be explained on the hypothesis (now well established on other grounds) that the reddest stars are the lowest in effective temperature. With the latest data regarding temperature and surface brightness, it appears that the fainter red stars are somewhat smaller, and presumably denser, than the sun, while the brighter ones are very much larger than the sun, and presumably of very small density. The latter class probably represent an early stage in stellar evolution, and the former the latest stage that can be observed. In the intermediate stages the star would be hotter, passing through orange and yellow to white, and back to red as it approaches extinction. On this hypothesis there ought to be two distinct kinds of red and orange stars. The distribution of proper motions among these stars supports this theory, showing that those of given apparent brightness may be divided into two groups—one of intrinsically faint stars relatively near our system, the other of bright stars remote from it.

PRINCETON UNIVERSITY,
April 20, 1910.

SPECIES IN AGAVE.

(PLATES XXXII AND XXXIII.)

BY WILLIAM TRELEASE.

(*Read April 22, 1910.*)

As Dr. Gray once said, species are nothing but human judgments (he even added very fallible judgments as some of us know to our sorrow), and as such they have changed and may be counted on to change with the minds that frame them, oscillating about the truth in a series of approximations to a definition of the also—but less rapidly—changing forms of living nature. A glance at the work of their makers shows that they have always been obscured by insufficient knowledge of real differentials, and even in the masterly synopses of Linnaeus and other epitomizers too few of them have usually been known to permit characters to be so framed as unquestionably to exclude those to be revealed by the exploration of new regions or by closer study at home. Botanists have rarely been able to build on the work of their predecessors without frequent reference to more than original descriptions, and in their effort to fix types they have been more helped by that uninspiring accumulation of dried plant remains, the herbarium, than by anything else unless indeed it be a well done illustration showing a pre-specific existence of a species—if such an expression may be used notwithstanding a sometimes handy convention that species are not to be sought earlier than the date of their formal binomial christening by the great Swedish naturalist.

Succulents have offered rather more than their share of trouble to those who have undertaken to describe and classify them, for one reason because they usually are, or appear to be, difficult of preservation in the herbarium. The fallacious notion, that, being easily brought in alive so that they may be grown in gardens, they are more surely preserved in this way for reference, may have had something to do also with damping the ardor of herbarium makers.

To this circumstance is attributable the fact that many species of this kind of plants have been described from garden specimens which have disappeared sometimes almost before the ink was dry on their descriptions, and that these have been drawn in many instances necessarily from easily transported rather than representative material. Very naturally, too, garden plants claiming specific recognition in a study of this kind have been accounted for though absolutely nothing was known of their source or origin; and descriptions are sometimes not free from at least the suspicion of being based on foliage of one species—possibly warped in character to ease its assimilation with an earlier description, flowers of a second, and possibly fruit of a third,—superadded in a laudable effort to complete the original account of a vegetating type, itself long lost.

Agave stands well to the front among genera exemplifying these difficulties, and it presents some that are almost its very own, because of having enjoyed a marked if transient garden popularity a little over a generation ago. Linnæus, in 1753, named only four species, two of which are now accredited to other genera. Twenty-five years ago, after excluding a large number of nominal species, Mr. Baker admitted 127 true agaves; and more than 200 species, of which many are nondescripts, must now be admitted to even a conservative list. Over one third of those recognized by Mr. Baker were based on vegetating plants—and on garden specimens at that; and most of the many others that he relegated to a synonymic place, as well as those that he had to lay aside as unidentifiable, had been described from garden plants, often of only a few years' growth.

In a study of the genus to which I have been devoting such time as could be spared for the last ten years, an effort has been made at once to understand the spontaneous representation of this genus—characteristic chiefly of the Mexican plateau, and to exhaust all possibilities of identification with nominal garden species before accepting as new to science even the most striking form met with in nature. During the fad for cultivating agaves, beginning about forty years ago, large prices were sometimes paid for good specimens. Although dealers and collectors never showed an undue zeal

to reveal the location of the mines from which they were drawing wealth, the fact that the traffic was large and profitable is responsible for the preservation of some disjointed scraps of information that may now and then be pieced together into clues that show where some of the most extensive and varied collections were made, and thus indirectly ensure reference to wild plants for species based on long lost garden specimens. It is with no small satisfaction that by means of such a devious argument I have been able to follow in the footsteps of the collector Roezl, and to understand at least a part of the species of his collecting that had otherwise passed into troublesome uncertainty; and no opportunity has been missed to examine the precise locality indicated as having been visited by botanists whose writings or collections have entered into the history of the genus—as, for instance, the lava beds on which Schiede found one of its most persistent stumbling blocks, *Agave lophantha*, and one of its good but long-discarded other species, *A. obscura*.

In such a study, essentially an honest effort to see and account for the forms actually presented by nature, so that others may see and know them, one must always be moulded by the times that he lives in. If unable to apply a unit-character criterion in discriminating between species, I find myself equally unable to adopt so broad a gauge for their measure as to join under one name the manifold West Indian forms in which so good a botanist as Grisebach could seen only the century plant. I find, however, that in these plants, long-lived, slow-growing, and even in the field most commonly seen only in their vegetative dress, a successful study calls for attention to minutiæ that, being unnecessary for segregations in most groups, receive there less attention than they really merit, and are often looked on with suspicion when used. Ascertaining the stability and significance of these proves at once a fascinating and disturbing part of the study.

In fruit, flower, pedicel, bract and scape characters, the agaves do not offer variation or differentiation very unlike what is usual in other genera of equal range and size; but in many cases in which such characters are still unavailable, those drawn from the leaves, and utilized in the description of species from young garden

specimens a generation ago, prove, within limits, entirely dependable. A few illustrations—from very many that might have been selected—will render this clear:

If the end-spine of *Agave americana*—as it occurs in our gardens everywhere, green, striped, yellow-margined or yellow-centered—had been attentively studied years ago, as it has been recently, in comparison with that of the now almost equally common yellow-margined *A. picta,* the former species would never have been made to include the latter (Pl. XXXII.). The spines of gray henequen (*A. fourcroydes*) and green sisal (*A. sisalana*) supplement other characters in segregating these constituents of what is still too often called *A. rigida;* and in this respect *A. angustifolia* differs so greatly from either as to make one who knows the differences wonder how, under whichever of its aliases it was encountered, it ever could have entered into this same modern complex called *rigida* (Pl. XXXII.). Three groups—superspecies, they might be called—of the now economically interesting zapupe agaves are distinguishable from one another, even to the touch, in this same character (Pl. XXXIII.),[1] and each group falls into species on its marginal arming. The likewise important group of mezcals grown for the production of Tequila spirits, known to science in one compositely described species (*A. tequilana*), shows a similar differentiation into an even greater number of forms (Pl. XXXIII.); and many of the great maguey forms grown all over the Mexican tableland for the production of pulque are unmistakably distinguishable on their spine and prickle characters.

These examples, I trust, may justify the devotion of a somewhat lengthy prologue to the argument that small things are not to be despised; or to a short epilogue drawing the conclusion that the arming of an *Agave* is no less significant in species discrimination than the disarticulation of the sepals of an apple, and that, in fact, neither stands alone.

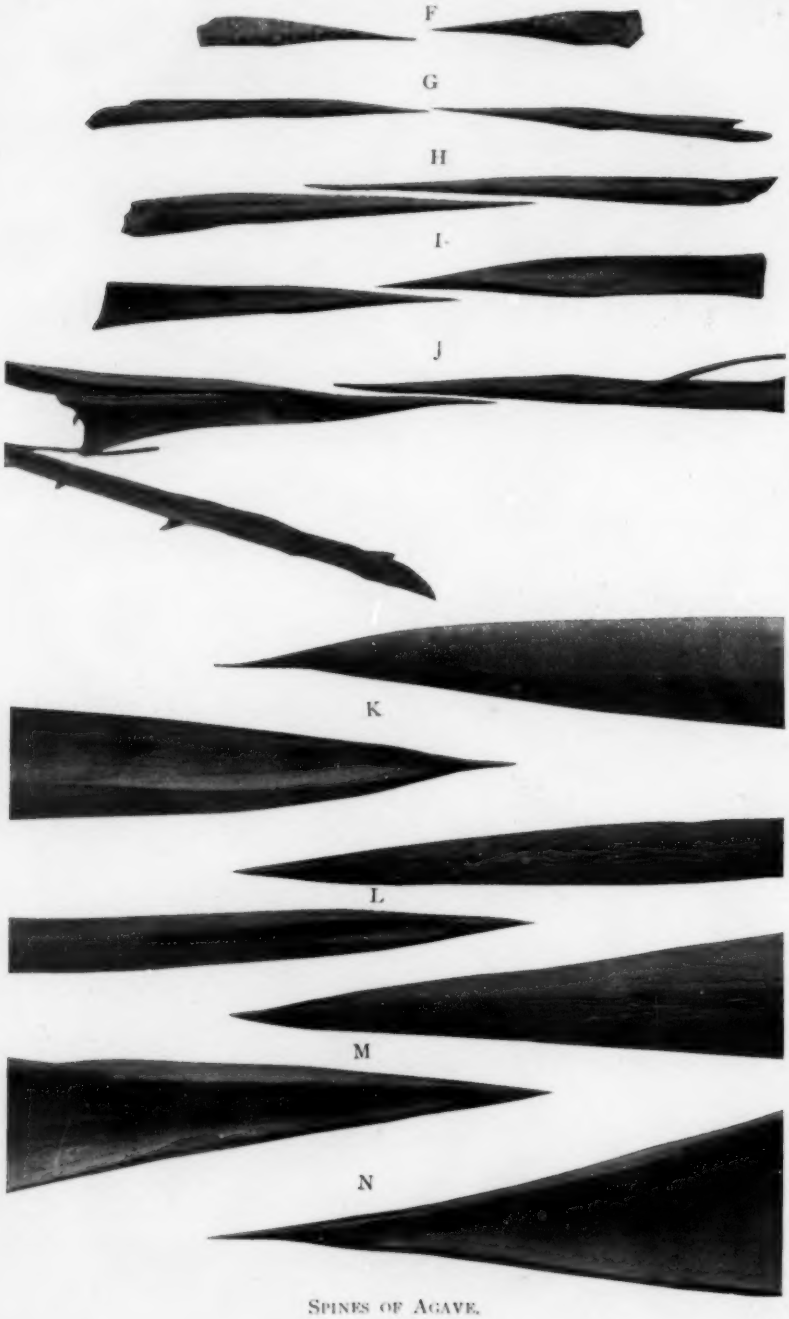
Until within very recent years, few herbaria have possessed more than one or two leaf fragments of a given *Agave,* and where more than one occurred the chances were good that they were not co-

[1] *Trans. Acad. Sci. of St. Louis,* XVIII., no. 3, May, 1909.

specific, even if either of them might properly bear the name attached to it. Jacobi clearly saw the value of spine characters in this genus, in his study of its representatives in the garden collections of his day, and he applied them as consistently as he could in his descriptions; but unfortunately the material on which these were based was often immature and the descriptions are too frequently generalized. Of the many illustrations of *Agave*, sometimes exquisitely colored, almost none approximates the truth in these details any more closely than a studio-made volcano approaches the true declivities of its cinder-cone and foot-slope. Notwithstanding all of its defects—some of them very real and serious—photography now ensures the truthful picturing of minutiæ that the eye of the describer may mistake and the pencil of the delineator is quite likely to misrepresent. Even by its aid, however, part truths may appear as truths, and a real fact may enter as an unreal specific character. It is for this reason that my own conception of specific identities and differences in this genus oscillates as my study proceeds: the leaf characters of a first specimen being most commonly ignored until more and different material forces their recognition; but with increasing evidence that, chosen from mature leaves of adult plants, and used with judgment, they are dependable.

Obviously, no species in any group can be considered as fully defined until all of its characters are known; and no species is satisfactorily described until its characters have been tersely brought together in contrast with those of its allies, as is painfully evident whenever a new form is confronted with published descriptions, which may be equivalent to saying that no species can be satisfactorily described until all of its closely related congeners are known. Obviously, too, species based on incomplete material are more likely to prove capable of ultimate subdivision than those of which all characters are represented when the first description is drawn; and in placing reliance on minutiæ, more than the usual need exists for using these with the judgment derivable only from experience, and for selecting with the greatest care their adult state. With such care, and subject to these restrictions, the species of this difficult genus, even in their vegetating form, appear to be capable

SPINES OF AGAVE.



SPINES OF AGAVE.

of clear delimitation; and in their essentials they show evidence of being much less mutable than they are commonly supposed to be when judged by the impression that they make on the untrained eye, or when differences in habit are given undue emphasis.

EXPLANATION OF NATURAL-SIZE ILLUSTRATIONS OF AGAVE SPINES.

PLATE XXXII. A, Three spines of *Agave americana.* B, Three spines of *A. picta.* C, Three spines of *A. sisalana.* D, Three spines of *A. fourcroydes.* E, One spine of *A. angustifolia.*

PLATE XXXIII. F–J, Two spines of each of the "zapupe" agaves: F, *A. zapupe;* G, *A. Deweyana;* H, *A. aboriginum;* I, *A. Lespinassei;* J, *A. Endlichiana.* K–N, Spines of Tequila mezcals—two each except the lowest: K, "azul"; L, "ziguin"; M, "mano larga"; N, "Chato."

GROUPS GENERATED BY TWO OPERATORS EACH OF WHICH TRANSFORMS THE SQUARE OF THE OTHER INTO A POWER OF ITSELF.

By G. A. MILLER.

(*Read April 23, 1910.*)

Two special cases of the category of groups defined in the heading of this paper have been considered, viz., when the square of each of the two generating operators (s_1, s_2) is transformed either into itself or into its inverse by the other.[1] In each of these cases it was found that the orders of s_1, s_2 do not have an upper limit. It will be found that both of these orders must always have an upper limit unless at least one of these operators transforms the square of the other either into itself or into its inverse.

The given conditions give rise to the following equations:

$$s_1^{-1}s_2^2s_1 = s_2^a, \quad s_2^{-1}s_1^2s_2 = s_1^b.$$

If at least one of the two numbers a, b were odd the order of the corresponding operator would be odd, and hence the group (G) generated by s_1, s_2 could be generated by a cyclic group of odd order and an operator transforming this group into itself. As many of the properties of such a group are known we shall confine our attention in what follows to the cases when both a and b are even numbers, and hence we shall assume that the conditions under consideration are written in the form

$$s_1^{-1}s_2^2s_1 = s_2^{2\alpha}, \quad s_2^{-1}s_1^2s_2 = s_1^{2\beta}.$$

Some fundamental properties of s_1, s_2 may be deduced from these equations in the following manner:

$$s_1^{-2}s_2^2s_1^2 = s_2^{2\alpha^2}, \quad s_2^{-2}s_1^2s_2^2 = s_1^{2\beta^2}, \quad s_2^{-2}s_1^{-2}s_2^2 = s_2^{2(\alpha^2-1)}s_1^{-2},$$

$$s_1^{-2\beta^2} = s_2^{2(\alpha^2-1)}s_1^{-2}, \quad s_1^{2(\beta^2-1)}s_2^{2(\alpha^2-1)} = 1.$$

[1] Cf. *Paris Comptes Rendus*, Vol. 149 (1909), p. 843.

From the last equation it follows that each of the two operators $s_1^{2(\beta^2-1)}$, $s_2^{2(\alpha^2-1)}$ is invariant under G and therefore it results from the first set of equations that

$$s_1^{2(\beta^2-1)} = s_1^{2\beta(\beta^2-1)}, \quad \text{or} \quad s_1^{2(\beta-1)(\beta^2-1)} = 1 = s_2^{2(\alpha-1)(\alpha^2-1)}.$$

By combining the last equation with

$$s_1^{2(\beta^2-1)} = s_2^{2(1-\alpha^2)}$$

it results that

$$s_1^{2(\alpha-1)(\beta^2-1)} = 1 = s_2^{2(\beta-1)(\alpha^2-1)}.$$

By transforming s_2^2 by $s_1^{2(\beta^2-1)}$ and s_1^2 by $s_2^{2(\alpha^2-1)}$ it is clear that the orders of s_1, s_2 must divide respectively

$$2(\beta^{2(\alpha^2-1)} - 1) \text{ and } 2(\alpha^{2(\beta^2-1)} - 1).$$

The orders of s_1, s_2 must therefore divide the highest comon factor of

$$2(\beta-1)(\beta^2-1), \quad 2(\alpha-1)(\beta^2-1), \quad 2(\beta^{2(\alpha^2-1)}-1)$$

and

$$2(\alpha-1)(\alpha^2-1), \quad 2(\beta-1)(\alpha^2-1), \quad 2(\alpha^{2(\beta^2-1)}-1)$$

respectively.

The subgroup (H) generated by s_1^2, s_2^2 is evidently invariant under G and the corresponding quotient group is dihedral. As the commutator subgroup of H is composed of invariant operators under G the fourth derived of G is always the identity.

§ 2. *Groups generated by two operators which satisfy one of the following sets of conditions:*

$$s^{-1}s_2^2s_1 = s_2^4, \quad s_2^{-1}s_1^2s_2 = s_1^4; \quad s_1^{-1}s_2^2s_1 = s_2^{-4}, \quad s_2^{-1}s_1^2s_2 = s_1^{-4}.$$

The first set of relations implies that $\alpha = \beta = 2$. Hence it results from the preceding section that the orders of s_1, s_2 must divide 6. If these orders are 6 H is of order 9 and s_1s_2 transforms the operators of H into their inverses. Hence s_1s_2 is of even order. If this order is 2 the group generated by H and s_1s_2 is of order 18, and it is completely determined by the facts that it contains the non-cyclic group of order 9 and an operator which transforms each operator of this non-cyclic group into its inverse. In this case G

is of order 36 and such a G can clearly be generated by the two substitutions

$$s_1 = abc \cdot de, \quad s_2 = ab \cdot def.$$

As we may annex to these substitutions any constituents of order 2 in new letters it results that the order of $s_1 s_2$ is an arbitrary even number and hence the order of G is an arbitrary multiple of 36. To prove that there is only one such group of a given order the following considerations are helpful. The cyclic group generated by $(s_1 s_2)^2$ has only the identity in comomn with H since $s_1 s_2$ transforms each operator of H into its inverse. This cyclic group is invariant under G. In fact each of its generators is transformed into itself by half the operators of G and into its inverse by the rest of these operators since $(s_2 s_1)^2 = (s_1 s_2)^{-2} = s_2^{-1} s_1^{-1} s_2^{-1} s_1^{-1} = s_2^2 s_2^{-2} \cdot s_2^{-1} s_1^{-1} s_2^{-1} s_1^{-1} = s_2 s_1^{-1} s_2 s_1^{-1}$. It is now clear from the general theory[2] of simply isomorphic groups that if s_1, s_2; s_1^1, s_2^1 are two pairs of operators satisfying the given conditions and if they generate groups of the same order these groups are necessarily simply isomorphic. Hence the theorem: *If each of two operators of order 6 transforms the square of the other into its fourth power these operators may be so selected that they generate a group whose order is an arbitrary multiple of 36 and there is only one group of each such order which can be generated by two of its operators of order 6 satisfying the given conditions. The second derived of each of these groups is unity.*

When the order of only one of the two operators s_1, s_2 is 6 that of the other must be 2, since it transforms the square of the former into its inverse. In this case H is the cyclic group of order 3 and the order of G is an arbitrary multiple of 12. The second derived of all of these groups is the identity since $(s_1 s_2)^2$ and s_1^2 generate an invariant abelian group and the corresponding quotient group is the four-group. When the order of s_1 is 3 that of s_2 must be 2 and G is the symmetric group of order 6. If s_1, s_2 are both of order 2 they may be so selected as to generate an arbitrary dihedral group. Combining these results we have that when s_1, s_2 are both of order 2 they may generate one and only one group of

[2] *Bulletin of the American Mathematical Society*, Vol. 3 (1897), p. 218.

a given order and this order is an arbitrary even number greater than 2; when one of these operators is of order 6 while the other is of order 2 they may generate one and only one group whose order is a given multiple of 12; when both of them are of order 6 the order of the group generated by them is a multiple of 36 and they may be so selected as to generate a group whose order is an arbitrary multiple of 36, but each such group is completely determined by its order.

If we consider all the possible groups which can be generated by two operators satisfying the two conditions under consideration it results from the above that there are exactly three such for every order which is a multiple of 36, one of these is dihedral, another is generated by an operator of order 6 and an operator of order 2, while the third is generated by two operators of order 6. When the order is divisible by 12 but not by 36 there are two and only two such groups, while the dihedral group is the only one that can be generated by two such operators whenever the order is any other even number greater than 2.

When s_1, s_2 satisfy the relations $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s_2 = s_1^{-4}$ their orders must divide 18 according to the general formulas of the preceding section. The following substitutions prove that these orders may be exactly 18:

$$s_1 = acegibdfh \cdot jk, \quad s_2 = abidecghf \cdot lm.$$

As $s_1s_2 = agd \cdot beh \cdot jk \cdot lm$ the order of the group generated by these two substitutions is 108. The smallest group that can be generated by two operators of order 18 which satisfy the given conditions is of order 54 and such a group is clearly generated by the two substitutions $acegibdfh \cdot jk$, $abidecghf \cdot jk$.

In general, s_1^3 is commutative with s_2^2 and hence with every operator of H. Similarly, it may be observed that s_2^3 is commutative with every operator of H. From this it results that every operator of the dihedral group generated by s_1^9, s_2^9 is commutative with every operator of H. It is also clear that these groups can have only the identity in common since the former contains no invariant operator of odd order. It therefore results that G must involve the

PROC. AMER. PHIL. SOC., XLIX. 195 P, PRINTED JULY 29, 1910.

direct product of H and this dihedral group. As these two groups clearly generate G it results that G is this direct product. That is, *if two operators of order* 18 *satisfy the conditions* $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s = s_1^{-4}$ *they generate the direct product of a dihedral group and a certain non-abelian group of order* 27. *Moreover, every such direct product can be generated by two operators of order* 18 *satisfying the given conditions.*

If only one of the two operators s_1, s_2 is of order 18 the other must be of order 9 since s_1^2, s_2^2 cannot be commutative. That is, if an operator of order 18 and an operator whose order is not 18 satisfy the conditions $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s_2 = s_1^{-4}$ these operators must generate the direct product of the group of order 2 and a certain group of order 27. When s_1, s_2 are both of order 9 they generate a group of order 27, and when both are of order 2 they generate a dihedral group. Combining these results we have that if two non-commutative operators satisfy the two conditions $s_1^{-1}s_2^2s_1 = s_2^{-4}$, $s_2^{-1}s_1^2s_2 = s_1^{-4}$ their orders have one of the following pairs of values 18, 18; 18, 9; 9, 9; 2, 2. In the first and last of these cases G may be any one of an infinite system of groups; viz., any dihedral group in the last case, and the direct product of such a group and a certain group of order 27 in the first case. In each of the other two cases there can be only one group, viz., the non-abelian group of order 27 which involves operators of order 9 in the third case, and the direct product of this group and the group of order 2 in the second case.

UNIVERSITY OF ILLINOIS,
December, 1909.

THE GREAT JAPANESE EMBASSY OF 1860.

A Forgotten Chapter in the History of International Amity and Commerce

By PATTERSON DUBOIS.

(*Read April 21, 1910.*)

The American trader with Japan, the traveller or sojourner in Japan, and the national representative or envoy to Japan, all find their transactions with the Japanese greatly facilitated by certain American ideas, patterns and methods now firmly rooted in Japanese practise.

First and foremost, the money system corresponds closely to that of the United States and the coinage itself is convenient, exact and trustworthy. In close accord with this, the American feels himself comparatively at home with the banking, postal and telegraph facilities, in public school aspects, and finally, he takes comfort in the assurance of an up-to-date dentistry and surgery.

But the international American little realizes how all these essentials of modern western life came to be imported from his own country and adopted by the keen-eyed Japanese as indispensable models for the Meiji or "enlightened rule."

The historic fact has dropped into almost total oblivion. This practical Americanizing of Japan harks back to the well-nigh forgotten visit of the great Japanese Embassy to the United States in 1860.

After the first assurance of friendly relations or amity, the rock-bottom of a stable, thriving and reciprocal commerce between two nations is to be found—as already indicated—in a scientifically exact and trustworthy coinage and system of exchange. Of this, more later. But first, as to the historical setting of one of the most picturesque and potent of all international events.

Americans delight to shout themselves hoarse in the praise of

Perry. We rightly glorify the tactful, gallant and picturesque manner in which he punctured the screen which shut out the western world from the eye of the Rising Sun. We have a sufficient feeling that he took the initative in securing friendly, to be followed by trade, relations in advance of all other nations—whatever slippery foothold the Portuguese, English and Dutch had at one time or another acquired only to lose again. We are sure enough that Japan's introduction to the circle of commercial nations was an American performance and that Perry took the initiative hand in it.

And there we stop. We know that American training goes back to Japan in the person of many a university graduate and we know of other influences, military, naval, industrial, commercial educational, professional, ethical, religious, which Japan has sought and obtained from both Europe and America. But we do not seem to realize that to certain impressions made upon the barbaric and wondering embassy of 1860 may be traced definite and continuous lines of development through the vitalizing of more or less latent Japanese powers and ideals. The work which Perry began in 1853–54 was only completed in the visit of the embassy of 1860 to this country. Indeed the embassage is the one signal factor in making the Perry incident permanently effective. Nor was it completed in the signing of the treaty in Washington, whatever might be officially held to the contrary. It is safe to say that the Japanese *learned more which has proved germinal in their re-making,* from their visit to the United States—but especially to Philadelphia, immediately following the diplomatic formalities in Washington, in 1860, than from any other one American source since Perry anchored in the Bay of Yeddo. Indeed I propose here to show such a historical continuity and persistence of certain formative elements of modern progress now accepted as the fixed order of things in Japan, and even incipiently in China, as can be traced to no other visible historical source than the embassage of 1860. And this, notwithstanding the complete revolution in the government and policy of Japan eight years later.

And yet, as I said at the outset, this embassy seems to have

passed almost into oblivion. The president of the American Asiatic Association in an article (1907, *The Outlook*) speaks of it as "one of several missions which about that time were sent by the Shogunate to other countries." In a general sense this is true, but more strictly the truth is that in Townsend Harris's negotiation of the commercial treaty in 1858 the Americans insisted that the first embassy to go out from Japan to any country should visit the United States first of all.

An English authority, Mr. J. Morris, in his able book, "Makers of Japan,"—to which I make acknowledgment—admits that "Modern Japan dates from the advent . . . of the American squadron under Commodore Perry in 1853;" and that the Perry compact was "the thin edge of the wedge." And yet he gives no indication of any knowledge of the embassy of 1860 for in noting the mission of the Marquis Ito abroad in 1871, he says that his was not the first embassage, "for a mission of two feudal barons had been sent to Europe in the early 'sixties."

Even admitting that this embassy was one "of courtesy" merely, as Dr. Griffis and others say, we shall see that something more vitalizing and lasting than courtesy grew out of it.

That the embassy of 1860 should have dropped so completely out of sight as an international event will doubtless lead many to conclude that at best it was but a spectacular fizzle entirely wanting in constructive elements or continuing effects. Others will remind us that from the invasion of Perry up to and after the first foreign embassage all intercourse was through the Shogun or "Tycoon" whose rule was becoming rapidly challenged and his authority in a large part of Japan denied. But the embassy was neither a mere courtesy, a spectacular fizzle nor was it of temporary import. Nor does the subsequent overthrow of the Shogun *in any degree vitiate the contention that specific visible lines of Japanese progress emerge from the personal presence of the "princes" and their retinue in the United States in 1860.*

On the part of Japan all international adjustments at this period were made by authority of the Shogun—better known at that time in the West as the "Tycoon." And who was the Shogun, or what

was the Shogunate? He was a military commander-in-chief, who had won his arbitrary power—or held it—as the strongest of the feudal barons. He was *de facto* ruler of Japan, his government being centered at Yeddo (now Tokio). The ruler *de jure* was the Mikado whose position was that of a sort of deified headship and an inert nominality.

The Shogunate began with the house of Tokugawa in 1603. It ended by the voluntary resignation of Prince Tokugawa Keiki in 1868 in the interest of national preservation and progress. For nearly two and a half centuries the Shogunate was the personification of a military supremacy in one of the most complete and exacting feudal systems known to history.

It was the Shogun Iyesada who became the treaty maker of 1854. It was the Ten-shi, or Mikado, the ruler *de jure,* Komei, who, under pressure from the Shogun's opposers, rallied his nerves to the extent of refusing to ratify these treaties even though he ultimately gave way. But he had a large official and popular backing. In fact the double-headed rulership had become a source of strife which threatened to disrupt the country. The opening of the door to the foreigner and the introduction of foreign methods were fast becoming the national dispute.

In spite of the expulsion of all foreigners, except a few severely restricted Hollanders, and the massacre of Christians in the seventeenth century, in spite of a universal espionage and lips sealed to foreigners, in spite of barbaric military standards and codes of honor—spears and swords outranking firearms—the scent of western enlightenment gradually penetrating the air quickened a new consciousness of unrealized power. Feudal rule had for some years given rise to murmurs and calls were heard for a full restoration of the Ten-shi, or Mikado, to real power. So Iyesada's treaties were made a plausible ground of opposition to the Shogunate even by the progressive clans who had introduced a number of foreign inventions. For the *purpose of getting rid of the Shogunate* the slogan of these very progressionists was *the expulsion of the alien.*

The Shogun adhered to his policy of admitting the alien (first forced by Perry) but as soon as the progressive clans succeeded in

overthrowing the Shogun's supporters they at once advocated the policy of opening the country to strangers, also. It is important to note this as it goes to show that although the embassy of 1860 was sent out by the boy Shogun, Iyemochi, yet the American impress which it carried back was in accord with that progression which ultimately triumphed even though the Shogunate was abolished.

Perry landed his men at the little village of Uraga in the Bay of Yeddo in July, 1853. For two hundred and thirty years no stranger had entered the feudal empire of the Rising Sun. Perry delivered, through messenger, President Fillmore's letter and sent word to the Shogun Iyeyoshi that he would return the next year for an answer. When Perry returned in February, 1854, Iyeyoshi was dead and Iyesada reigned in his stead, and the treaty of amity was signed, March 31, 1854, opening two ports to us, Shimoda and Hakodadi, for trade and all ports for ships in distress. As a matter of fact Shimoda was not opened but Kanazawa was selected as the first port to be opened to trade.

In 1858 the Shogun Iyesada died and was succeeded by a fifteen-year-old boy Shogun, Iyemochi, and his regent prince; and it was under this rule that the embassy of 1860 was sent to the United States. This period suffered from internecine strife on questions of alien influence, feudalism and dynastic ambition. Moreover, the Mikado was being pressed by certain feudal lords to close the ports, abrogate the treaties and expel the strangers. But the foreign powers were also pressing the regent to stand by the agreements. Then, just after he had sent out the embassy, the regent was assassinated. This was in March, 1860. The country was in a ferment but the great embassy was on its way across the Pacific.

The thin edge of the wedge inserted by the Perry compact in 1854 was chiefly one of amity and of hospitality to our seamen. Nevertheless it was to pave the way for closer commercial relations, through another treaty. This latter treaty was negotiated by our Consul-general Townsend Harris, July 29, 1858.

Mr. Harris had raised the United States flag over an ancient Buddhist temple at Shimoda September 44, 1856, and established our legation there, being "the first of the diplomatic representatives of

foreign powers," as Morris concedes, "to dwell in the newly awakened Land of Sunrise and the first to arrange a treaty of commerce." Harris had a hard time of it. President Pierce sent him out in 1855. He remained in seclusion for nearly two years at Shimoda—the tip end of the southeastern coast—before he could get an audience with the Shogun. Until he could do it in person he refused to deliver the President's letter. For about eighteen months he was without news from home—receiving neither letter nor newspaper. He entered Yeddo in November 1857, gained his long insisted-upon audience December 7, 1857, spent some months in parley and finally signed the treaty of commerce July 29, 1858. This treaty became the model for others between Japan and European nations. It is interesting, too, that when the British Earl of Elgin arrived for a similar purpose, Harris "lent" his secretary, Mr. Hewsken, to act as interpreter for the British envoys. Lord Elgin negotiated a treaty similar to Harris's and it was signed August 26, 1858. Thus the first British treaty was signed about six months after Perry's and the second one was signed one month after Harris's.

Now the agreement with Harris was that his treaty should be ratified in Washington. Under our treaties, however, no Japanese envoy was to go out until after America had been officially visited. After deferring the dreaded event as long as possible the Japanese finally applied to the United States for a man-of-war to transport the envoys.

It was on March 27, 1860, that the United States man-of-war *Powhatan* arrived at San Francisco, carrying the ambassadors and their immense retinue. After a few days of dining and sight-seeing the visitors reëmbarked for Washington by way of the isthmus—this being nine years before our east and west coasts were united by rail.

The United States man-of-war *Roanoke* awaited the orientals at Aspinwall (now Colon), a flourishing seaport and the Atlantic-side terminus of the forty-nine-mile railway connecting it with Panama, the entrepôt of the Pacific side. Aspinwall was on the *qui vive*. In anticipation the United States flag officer courteously invited the

British Rear-Admiral Sir Alexander Milne to come on board the *Roanoke* when the embassy should arrive. The invitation was declined, the rear-admiral subsequently remarking (so reported) that it was "a great farce, foolish and nonsensical." Nor would he raise his flag, though every other vessel did. His attitude gave the Japanese an unfavorable impression of the English nation—so reported later by one of the retinue.

Under command of Captain S. F. Dupont the ship *Philadelphia* left Washington, May 11, for Hampton Roads where the embassy was to be officially received. On May 12, at 9.30 p. m., the *Roanoke* arrived in the roadstead. She was boarded by Capt. Dupont; Capt. Taylor of the Marine Corps; Mr. Ledyard, son-in-law of the Secretary of State; Mr. Portman, the Dutch interpreter; Commander Lee (brother of Robert E. Lee and father of the late Fitzhugh Lee); Lieut. David D. Porter (later, commanded the expedition against Fort Fisher, later, Admiral); Mr. McDonald, invited guests, and reporters. After the formalities of presentation in the cabin of the *Roanoke* the treaty itself was exposed to view. Up to the present time it had been kept wrapped in a case of red cloth and sacredly secured in a superb lacquered chest about three feet long, eighteen inches wide, and twenty-six inches deep, never out of sight of a guard, and transported by poles resting on the shoulders of four men.

Two days later, May 14, the transfer of the entire company, box, bag and baggage, from the *Roanoke* to the *Philadelphia* was completed. The envoys and retinue of all grades numbered no less than seventy-six persons—the upper ranks gorgeously arrayed and plentifully begirt with swords. The luggage filled four cars on the Panama railroad. There were fifteen boxes of presents for President Buchanan and others. There was a beautiful "Sharpe's rifle" made by the Japanese as an improvement on the real American Sharpe presented to the Japanese by Commodore Perry six years earlier. The Japanese improvement consisted in an arrangement for cocking, priming and cutting off the cartridge, all at once. This has gone by now, but it was a forecast of Japanese aptitudes which we have seen illustrated in the late war. The money which

the orientals brought bulked immensely, too, for it consisted of Mexican silver and United States half-dollars.

Under command of Dupont the *Philadelphia* proceeded at once to Old Point Comfort, where for the first time Fort Monroe submitted to an almond-eyed inspection. There were no snap-shot cameras in those days, but the foresighted orientals had brought with them alert and skilled artists who immediately busied themselves making sketches of our boasted stronghold. And indeed these deft wielders of the brush were thus busy throughout the entire sojourn of the embassy among us. Who shall say that the many cartoons of things American which were thus officially carried back to Japan are not to be counted among the germs of a later expansion? Who knows but that these pictures helped to leaven the motif, eight years later, of the Emperor's edict at his crowning—"The bad customs of past ages shall be abolished and our government shall tread in the paths of civilization and enlightenment. We shall endeavor to raise the prestige and honor of our country *by seeking knowledge throughout the world.*"

On the same day, May 14, the embassy was received at the Washington Navy Yard by the commandant, Capt. Franklin Buchanan—the man who, less than two years later, commanded the *Merrimac* in her destructive work, in which she was finally vanquished by the little *Monitor*.

The landing of the embassy at the Washington Navy Yard was a brilliant and imposing spectacle. From the navy yard the orientals were driven under military and civic escort to Willard's Hotel—then the center of Washington's social gravitation. It is not necessary to the argument to enlarge upon diplomatic details. Suffice it to note that the ambassadors and attaches, eight in all, on May 16, were driven to the Department of State, where letters were presented to Secretary Cass in Japanese, Dutch and English. All communication was done through two interpreters—Namoura-Gohajsiro, the Japanese who spoke Dutch, and Mr. Portman, the Hollander who spoke English.

The next day, May 17, the ambassadors presented the Tycoon's greeting to the President, of which the following was the published translation:

To His Majesty, the President of the United States of America. I express with respect: Lately the Governor of Shimoda, Insooye-Sinano-no-Kami and the Metske-Iwasi-Hego-no-Kami (Prince of Idzu), both Imperial Commissioners, had negotiated and decided with Townsend Harris, the Minister Plenipotentiary of your country, an affair of amity and commerce, and concluded previously the treaty in the city of Yeddo. And now the ratification of the treaty is sent with the Commissioners of Foreign Affairs, Simme-Buzen-no-Kami, and Minagake-Awzi-no-Kami to exchange the mutual treaty. It proceeds from a particular importance of affairs and a perfectly amicable feeling. Henceforth the intercourse of friendship shall be held between both countries and benevolent feeling shall be cultivated more and more and never altered. Because the now deputed three subjects are those whom I have chosen and confided in for the present post, I desire you to grant them your consideration, charity and respect. Herewith I desire you to spread my sincere wish for friendly relations and I also have the honor to congratulate you on the security and welfare of your country."

(It would be easy to improve the English of this translation, but I give it as it was rendered at the time.)

The third envoy, unnamed in this letter, was Oguri-Bungo-no-Kami, and was known as the censor (or spy); following in order were the treasury officer who had full authority over the finances of the embassy, the governor (or executive manager), aids, interpreters, doctors, guards of the treaty box, and servants including "spies"—in all, seventy-six. The three "princes" who head the list as ambassadors were of equal rank with those who negotiated the treaties with Perry and Harris. They were not hereditary princes of the blood but Samurai members of the Tycoon's foreign affairs council.

It will be remembered that when Harris signed the treaty of commerce in 1858 it was stipulated that the ratifications should be interchanged in Washington. The formalities of the ratification of Harris's treaty were now in order. That this was a matter of moment to both nations and regarded as more than a picturesque affair of good feeling will become apparent. This is what President Buchanan said about it in his annual message to Congress seven months later (1860):

The ratifications of the treaty with Japan concluded at Yeddo on the 29th July, 1858, were exchanged at Washington on the 22nd May last and the treaty itself was proclaimed on the suceeding day. There is good reason to expect that under its protection and influence our trade and intercourse with that interesting people will rapidly increase.

The ratifications of the treaty were exchanged with unusual solemnity. For this purpose the Tycoon had accredited three of his most distinguished subjects as envoys extraordinary and ministers plenipotentiary who were received and treated with marked distinction and kindness both by the government and people of the United States. There is every reason to believe that they have returned to their native land entirely satisfied with their visit and inspired by the most friendly feelings for our country. Let us ardently hope, in the language of the treaty itself, that "there shall henceforward be perpetual peace and friendship between the United States of America and His Majesty the Tycoon of Japan and his successors."

Three weeks of state formalities, sight-seeing, and social functions—including the President's dinner on May 25, brought this first stage of the embassage to a close by a formal leave-taking in the blue room of the White House, June 5, at which a large gold medal was presented to each of the princes. But is this all? Is there enough in these three weeks of state and social function to generate definite lines of development and to vitalize latent powers and ideals such as we wonderingly view in these latter days? No. The embassy had one other official commission which could be fulfilled only in Philadelphia. *It was in the matter of money and exchange.*

After a brief stop in Baltimore the orientals arrived Saturday, June 9, in Philadelphia. For weeks the city had been stirred to its depths planning and making arrangements for the unprecedented event. The industrial metropolis had much to show to a nation so skilled in artizanship as the Japanese. Best of all, here was the mint, which, after treaty formalities, was *the* chief focal point of the visit to the United States.

The train drawn by an engine wrapped in the Japanese and United States flags, arrived in the afternoon at the old "Baltimore Depot" at Broad and Prime Streets (now Washington Avenue). Here the envoys with Captain Dupont and Lieutenant Porter, were met by Mayor Henry, members of council, judges and other public men numbering about two hundred—and dense crowds of the populace. The long procession of nearly a hundred carriages filed up Broad Street under an imposing escort of about two thousand cavalry and infantry led by General Robert E. Patterson and staff, and greeted by the din of popular huzzahs. Thousands of eyes were strained to glimpse the novel type of physiognomy peering

curiously out from the carriages. The Japanese artists were busily sketching as they rode our streets. Extraordinary curiosity arose from the closely drawn shades of one of the carriages, which eventually gave rise to a rumor that the occupant was in disgrace because he had "stopped watching the treaty box"—its sacred import having been already heralded by the newspapers. The procession brought up at the Continental Hotel where the embassy was regally entertained. The police carried the treaty chest into the hotel and guarded it under lock and key. During the week's sojourn in Philadelphia, crowds surrounded the hotel straining for a vision of the orientals as they appeared at their windows, but, said the reporters, the people "couldn't see a mite of difference between them and negroes!"

The Japanese were not unaware of the unique reputation of the Philadelphia Mint. All its early officers had been scientific men or high mechanists and not politicians. Politics had but recently invaded the management but the assayer and his assistant in a peculiar sense had given the mint a world-wide reputation for specialized accuracy. Their published works and the unvarying maintenance of the standard fineness of the coinage, for over a quarter-century of their incumbency, together with their comprehensive knowledge of the whole intricate operation of minting, had been a highly important although a popularly unapprehended factor in the national credit. For a nation, then, that had in itself the seeds of progress, a rising curiosity as to western methods, a tendency to catholicity, a receptivity to impressions, and an adaptive originality—as the Japanese had, notwithstanding a long isolation followed by internal strife—for a nation of this sort, America, and more particularly Philadelphia and especially the Mint, was the place to come.

On the morning of June 13 the envoys were received at the mint. In his address of greeting the director said that an international coinage was not likely to be realized, but he advised the foreigners to adopt the American standard. A mutual knowledge of the currency of both nations would promote commerce as well as friendly relations.

Preliminary formalities being over the occasion was so extra-

ordinary that a messenger was quietly despatched with orders to go to a nearby grammar school and procure a release for the day of three pupils, sons of the assayer and his assistants, bring them to the laboratory where their education might take a rare turn in a leisurely and close contact with the ambassadors and their attendants. I was the youngest of those three boys, and I am now drawing on memory which is both stimulated and held in leash by certain mementos, personal notes of the assistant assayer, various public prints and official documentary memoranda.

Entering the little "weigh room" attached to the laboratory the astonished youths saw the first and third ambassadors (the latter known as the censor), a stout functionary known as the governor, two interpreters—one Dutch and one Japanese—and perhaps one or two attachés, and a "prince" travelling unofficially for pleasure but in company with the embassy.

The high dignitaries were superbly dressed in silk brocaded, or inwoven, with gold, the pajamas or trousers being figured in exquisite patterns and as wide as skirts; the body-covering a loosely crossed waistcoat over which was a kimono or long loose coat. The paper handkerchiefs were carried in the crosswrap of the waistcoat. The fore part of the head was shaved while down the center of the crown a tightly twisted lock or short braid lay glued stiff and tied with white cord. The sandals were held in place with white silk cords. The ambassadors wore three swords, the lesser officers two—a sign of the Samurai or ruling military caste and a badge of honor now abolished.

As the boys entered, one of the expert under-assayers was seated at the delicate balance explaining the process. This was addressed to the Hollander, Mr. Portman, who in turn addressed the Japanese interpreter, Namoura, in Dutch. Namoura, in turn, translated the Dutch into Japanese and addressed the third ambassador who, in turn, communicated the information to the first ambassador —who received it with the utmost imperturbable and silent gravity. Whether he took it all in or not we boys could not tell.

The censor or third ambassador, Oguri, and the governor, Narousa, fat and spectacled with heavily rimmed glasses, were the

active investigators. Oguri produced a sort of steelyard of ivory with which he proceeded to test our weights against his own. The censor also brought forth an *abacus* or counting machine consisting of fifteen rows of wooden buttons sliding by five on parallel wires. Quite in accord with these crude and antiquated methods was the demand that the gold *cobang* (about as big as the palm of the hand) should be assayed not by a sample cut from it, but by consuming the entire piece. The chief assayer, Jacob R. Eckfeldt, and his assistant, William E. DuBois, were for the moment nonplussed by so extraordinary a request. High accuracy demanded very small samples and their department was nothing if not scientifically precise. But the heathenish proposition was uncompromising and the whole *cobang* it must be. When, after two or three hours, the gold had been separated and weighed and its fineness thus ascertained the envoys were not satisfied. They must next know exactly of what the alloy was made up. Sure enough, we see in this very demand an indication of what we have seen since—thoroughness.

Three *cobangs* were thus tested and to satisfy the strangers, also a United States gold dollar. Notwithstanding the cumbrous method of using an entire piece the results were exceedingly satisfactory—the *cobangs* running about 572 parts fine in 1,000. (It should be stated that while the embassy was yet in Washington a number of their coins were forwarded to Philadelphia to be tested in advance of the embassy's inspection. There were about twenty-five pieces in all, gold and silver *itzebus* and gold *cobangs*. They ran with a fair approach to uniformity.) At intervals, during the long operations the orientals took a short squat on their heels to smoke their tiny pipes. A luncheon was served to them in the mint when the display of chop-stick skill—picking up one pea at a time at high speed with two sticks, for instance—was recreating to observant America.

After this the envoys requested the mint officers to meet them at the Continental Hotel and hold a conference on matters relative to a comparison of Japanese and American coinage and means of exchange. Accordingly, Director Snowden and his clerk Linderman; the assayer, Eckfeldt, and his assistant, DuBois; and the

Melter and Refiner, Booth, repaired thither in the afternoon. Exactly what took place there we do not know. But the indications are that it was a "campaign of education" and that it was a spoke in the wheel of closer and fairer monetary relations, including the possibility of international coinage—in which movement Assayers Eckfeldt and DuBois were conspicuous promoters. The next day the embassy was at the mint again and the work continued.

A carefully written account at the time said:

> The very intricate business connected with the currency question between the United States and the Empire of Japan (the adjustment of which is one of the principal objects of the embassy) and which had been theoretically explained by the officers of the Treasury Department at Washington has been *solved to the satisfaction of the envoys by the assays performed in their presence at the mint.* The importance and value of this very desirable result cannot be overestimated and the thanks of the country are due to the officers of that institution for the very skilful manner in which they have discharged the duties imposed upon them by the government at Washington in connection with this business.

The tests having been concluded on the second day the envoys were again formally addressed by the director, who presented to them a certified copy of the results and also a full set of the current United States coins handsomely cased. The censor replied, thanking the officers of the mint for their courteous attention, expressing satisfaction with the results, and promising to lay the whole matter before his government *so that a system of exchange could be arranged between the two countries.*

A slight digression may be permitted just here to illustrate the need for such adjustment of exchange if commerce was to be encouraged by the treaties. I quote from the private note book of my father, then the assistant assayer of the mint. It was written the next year—1861.

> Before the Japanese Embassy started for this country a silver *itzebu* was coined, which was intrinsically one third of the Mexican dollar. (The itzebu was the monetary unit of the empire.) About the same time, for Hakodadi only, a northern port where American trade chiefly centered, they coined a *half itzebu* (so stamped on its face) whose weight was equal to half a Mexican dollar (a little over in fact) and the alleged fineness the same. The object of this was to meet a treaty provision which made Mexican dollars interchangeable with silver *itzebus,* weight for weight. By

this rule they would take from Americans a thousand dollars, and give in exchange two thousand of these new coins, being the equivalent in weight. But when the Americans came to buy goods with the coins they were informed that their current value was only a *half-itzebu*, according to the stamp. That is to say, a Japanese could *pay* them to an American, at the rate of two to the dollar; but could only *receive* them at the rate of six to the dollar. And if offered their *itzebu* (one-third dollar) they did not want it—had enough of them! Thus, there was a *half-itzebu* worth fifty per cent. over the whole *itzebu* for the sake of a shallow artifice. The embassy did not bring the former piece (the *half-itzebu*) with them but only the latter (the whole *itzebu*).

The *half-itzebus* were not equivalent in fineness although they were over-full in weight. They assayed 846 thousandths fine as against 900, and weighed 210.0 grains as against, say two tenths of a grain, over a half-Mexican dollar.

Thus, there was need for a basis of exchange. Moreover, the coinage had become debased, the feudal lords had secretly issued money, and the country was flooded with counterfeits. Paper money had depreciated and finance was unsound.

It has been shown in the earlier portion of this paper that at the time of the embassy Japan was in a state of internecine strife, that the Shogunate had been undergoing changes, and that a few years later (1867–8) the crisis came in which the real government was restored to the Mikado and the feudal Shogunate abolished. Just prior to this, however, in 1866, the Mikado, or Emperor, had ratified all foreign treaties of the Shogunate, and they continued in force. In 1870 the distinguished statesman, the Marquis Ito, was sent to the United States to study monetary methods and coinage standards. While here he wrote a memorandum on "Reasons for Basing the Japanese New Coinage on the Metric system." During Ito's absence from Japan the Hongkong mint was purchased and with British aid carried over to Osaka. Ito wrote home recommending the ultimate adoption of the gold standard although not yet quite feasible. However, the metric or decimal system was adopted and the currency closely conformed to that of the United States, the *yen* being the equivalent of our dollar and the *sen* the equivalent of our cent. It cannot be doubted that the first experience of the Japanese with the working of the metric system was obtained in our mint, as

the assay process is based on it and the money scheme is decimal, while the British scheme is not. Neither can it be doubted that the impressions carried back by the envoys in 1860 were factors in Ito's mission ten and eleven years later under a new government.

Ito was one of the progressives from Perry days, who (as previously explained) assumed an anti-occidental complexion merely for the sake of opposing the Shogunate. Prince Iwakura who headed the embassy of 1872, was another of those connecting links. He had not been favorable to foreigners but yet he had incurred the enmity of the Shogun's opposers. Under the empire he became one of the ablest of the emperor's advisers and was especially interested in the revision of the old treaties, which it took years to accomplish. Another was Matsukata, the father of the gold standard in Japan and the introducer of the metric system; and still another was Shibusawa, the father of the national banking system.

In 1872 Ito was here again, this time on a mission of which Prince Iwakura was the head. In an eloquent address in San Francisco, Ito said: "A year ago I examined minutely the financial system of the United States and every detail was reported to my government. The suggestions then made have been adopted and some of them are already in practical operation." The result was as already noted. Banking and even a postal system on American models also followed in this year.

That the lessons of 1860 were active forces in this later day is further evidenced in a private letter to the heads of the Assay Department from Dr. H. R. Linderman, previously the chief clerk of the mint in the days of the embassy and later a treasury agent in Washington versed in mint matters. This letter requested that the two under-assayers do him the favor to prepare as early as convenient a brief description of the processes in use at the mint. "I desire this information," said the letter, "to incorporate in a paper which I am preparing for the Japanese at the request of the department. They shall have due credit for their work and will place me under obligations." This was in March, 1871—the very year of the enactment of Japan's new coinage law under the pressure of Ito and Matsukata.

The two young under-assayers were the schoolboys of 1860, Jacob B. Eckfeldt (the present chief assayer) and the present writer, whose recollections of the envoys of the Shogunate were still vivid in their memories.

In a very real sense, then, Ito had become the connecting link and the effectuating successor of the envoys of 1860, anti-Shogunate as he had been, and now under the new Imperial regime, as he was. Notwithstanding the great break with the past on the incoming of the Meiji or "Enlightened Rule," the restoration to power of the Mikado-Emperor, there was an efficient continuity of the westernism inaugurated officially by the Shogun. The treaties of Perry and Harris and the Washington ratification still held in spite of various attempts to discredit and revise them, even in the seventies. Not until 1894 did such revision take place. To the credit of the Shogunate in its later days of the sixties be it said, the degenerated condition of its antiquated and heterogeneous monetary system and coinage was realized. It was seen with alarm that western commerce under the treaties was suffering and would suffer unless there was reform in the coinage. Hence the capital importance of the visit of the embassy of 1860 to the mint at Philadelphia.

The story revealed in these last few paragraphs, however, shows that the spirit of the Shogun and the impressions carried home by his now almost forgotten embassy remained as a wholesome and permanent leaven. Under Ito, Matsukata, Iwakura and others, the impressions carried home by the first embassy came to fruitage not only in an Americanized monetary and coinage system but in what lies deeper than these—the moral standard of a trustworthy precision in the manufacture of coin in which the Philadelphia mint has led the world. That this principle and achievement had become an ambitious scientific and commercial *motif* in new Japan is further evidenced in other ways. For instance, in 1875 sample coins were sent from the imperial mint at Osaka to our Department of State with the request from the Japanese government that these coins be carefully tested at the Philadelphia mint. The result of the test (in which I myself had an active part) was very satisfactory.

Similarly, we received for assay through the Japanese legation gold and silver coins in 1876, 1877, 1878, 1879, 1880. These were what are known as pyx coins, which are selected at random through the year for an annual test. They were invariably close to the standard, tending somewhat to run over rather than under it. Thus the pace set before the embassy of 1860 was the pace which the mint at Osaka under our stimulus was setting for itself. The Enlightened Rule recognized that a nation's position among commercial nations rests in very large degree on the confidence to be placed in the scientific precision of its coinage.

Leaving now this great essential result of the first embassy, let us look for other indications of its influence in the making of a new Japan.

While in Philadelphia the two physicians attached to the embassy, Measaki and Moryama, together with the governor Narousa-Genosiro, and the interpreter, attended an operation for lithotomy performed by the distinguished Dr. Samuel B. Gross, at the patient's residence. The anaesthetic was administered by the famous Morton himself, the discoverer of sulphuric ether for this use. The whole performance was a revelation to the orientals. They smelt and poured the ether on their hands, astonished at the coldness resulting from its evaporation. After the operation they carefully examined the instruments and showed so much interest in the whole subject that they were invited to attend the Jefferson Medical College, in which Dr. Gross was a professor.

In an address delivered before the students of the Jefferson in February, 1906, Baron Takaki, Surgeon General of the Japanese Navy, said:

> Japanese surgery is founded on the teachings of Dr. Samuel D. Gross for so many years surgeon in the splendid medical college in which we are gathered. Dr. Gross' "System of Surgery" translated into German was taken by my countrymen and retranslated into Japanese and upon that has been built up Japanese surgery as practiced to-day.

The Baron said that the thanks of the Japanese nation are due to the medical profession in this country, and added,

> The United States has been our teacher. We have tried our best to prove our faith in your teachings and doctrines by effective applications of your principles in safeguarding the health of our people.

In view of the impression made upon the doctors of the first embassy by Dr. Gross himself it is hardly possible that his name was unknown in Japan until his book was carried there from Germany. Undoubtedly this is another one of those cases of the long slow-but-sure working of American leaven through many political vicissitudes.

It is inadvisable to prolong this paper for many further details of the doings of the embassy during the week in Philadelphia. Suffice it to note, in brief, that among the places visited were Johnson's type foundry—where the orientals were presented with a book of specimen types and cuts and a silver mounted case of type; M'Allister's optical and philosophical instrument establishment where they witnessed experiments with air pumps, electrical machines, etc., and a lantern exhibition in which the Drummond light excited great curiosity; to the great foundries of the Merricks and of Morris, Tasker & Co.; to the gas works, where "grand stands" had been erected and were filled with hundreds of invited Philadelphians of both sexes—chiefly to witness the ascension of two great balloons (or rather to see Japanese for the first time behold aërial travel); to Bailey's jewelry establishment where, after examining with magnifiers the works of watches, they ordered a lot of them to be sent to their rooms, and where they showed judgment in the purchase of opera glasses, appreciating the chromatic lens, and caring little for such merely ornamental work as their own artificers could equal or excell. They chose the plain and the useful, displaying a keen selective sense. Here, too, the envoys were presented with a medal especially designed and struck by the Bailey house to commemorate the occasion. In addition to these places visits were paid to Baldwin's locomotive works, Sellers's machine works, the water works, and Girard College.

The Japanese were loaded with all manner of specimens of tools, instruments and products of our skilled workers—including pictures of the Baldwin engines, stereoscopes and views, a superb Sloat sewing machine encased in wood from Mount Vernon and (so said) the Treaty Elm; a Disston saw, level, gauge and square; a set of teeth on a gold plate, and many other samples of American originality, skill and enterprise.

The mention of the set of artificial teeth on a gold plate suggests a pendant to Gross' surgery. The modern development of Japanese dentistry is wholly American. According to Dr. Chiwaki, the president of the Tokio Dental College,[1] dentistry, as an art, is about two centuries old in Japan. In some respects the old dentistry was barbarous and crude but artificial dentures were made of carved wood—also of alabaster or ivory riveted to a base of hard wood. But on the whole the art was clumsy and its pursuit became disreputable.

"Perry's feat, however," says Dr. Chiwaki, "brought about a many-sided change in the political, social and educational institutions of Japan; and, in consequence, the old system of dentistry could not remain unaffected." Two American dentists, Dr. Eastlake and Dr. W. St. George Elliott, opened offices at the beginning of the Meiji era. This was "the first direct cause of the development of our dentistry in the true sense of the word." Others followed, and the Japanese came to the United States for dental education. Japan now manufactures dentist's appliances and supports at least two dental colleges.

The introduction of American dentistry to Japan is not directly traceable to the embassy of 1860 but its acceptance is one of those forms of Japanese confidence in American models first gained as a national leaven in the days of the Shogun.

For the sake of completeness and also to note two or three incidents or facts of contributory import in estimating results this study must follow the embassy out of Philadelphia to New York, Saturday, June 18, where there was repetition of street procession and general ovation as in Philadelphia. (On this very day the news of the assassination of the regent arrived by letter to the New York *Tribune*.)

According to an account in the *Tribune* the street scenes on the route of entry from the Battery to the Metropolitan Hotel on Broadway, were free from those "riotous excesses" which characterized the multitude at Philadelphia. Never, said the *Tribune*, had more human beings been congregated at and below the Battery

[1] *Dental Cosmos*, October, 1905.

than the envoys found awaiting them as the boat from South Amboy arrived. But the Metropolitan Hotel was at no time so riotously besieged as was the Continental in Philadelphia.

Barring the visits to two or three manufacturing establishments, the time was chiefly occupied with social functions, shopping, boat excursions, theaters and in packing the mountainous wares which they had bought and which had also been lavishly bestowed upon them largely for advertising purposes. In time, the envoys and lesser officers acutely discerned that they were being exploited. Many invitations were declined. Finally, so indecorous a pressure was put upon them to visit the opera in spite of their resolute declination that a serious affray was narrowly averted.

The embassy, having grown weary of their spectacular exploitation in New York, resolved to cut Boston and Niagara out of their program and set sail for Japan as soon as possible. They accordingly departed by the largest of our naval fleet, the *Niagara,* on Saturday, June 30, first steaming around the world's wonder, the *Great Eastern,* which had arrived only two days before and which now succeeded the Japanese as a popular ferment.

In the retrospect: Those were stirring times. The greatest ship in the world had crossed the Atlantic, Garibaldi had just taken Palermo, Lincoln had been nominated, and the Democratic Party had split into the Douglass and Breckenridge factions. The ocean cable itself was only two years old; the John Brown insurrection had occurred only nine months before; Mr. Lowe, the aëronaut, was planning a balloon voyage across the Atlantic; and the Prince of Wales was soon to be entertained.

The New York *Tribune* gave up two pages of small type to a description of the voyage of the *Great Eastern,* and Mr. Greeley editorially declared her to be a wonder without much maritime significance for the simple reason that only three or four harbors in the world could receive so huge a ship. The same big-brained, generally level-headed editor was unable to attach any practical importance to the visit of the Japanese. He saw through New York eyes and thus rhetorically delivered himself:

> If they [the Japanese] have the acuteness to see, as possibly they have, the uses to which they have been put, to gratify the inordinate vanity, the

inordinate greed, and the inordinate folly of those with whom they have come chiefly in contact, and if they believe that in these they see reflected the character of the whole people, then heaven help our reputation in Japan when these sons of hers go home. But let us hope they did not understand. In the simplicity of their natures and manners let us trust that they have gone back to their own country impressed not only with our material superiority but believing also that in all Christian graces, in the amenities of social life, in the refinements of personal good breeding we are unmeasurably their masters. . . . Of almost all that an intelligent traveller would like to be informed they have gone away as ignorant as they came. . . . Against the acquirement of all useful knowledge except in a few rare instances which make the rule more apparent, they have been sedulously guarded and the opportunity lost which will never recur again of impressing a people eager in the attainment of the arts of peace, with the true source of the wealth and power of Christian civilization.

Another New York paper thus commented:

They are small of stature, tawny of complexion, sleepy and feeble in their physical appearance and habits, and with only those characteristics calculated to excite a momentary curiosity.

The Philadelphia view was different. *The Inquirer* said:

They saw the triumphs of science and art made subservient to the comfort and happiness not to special classes merely, but to all. They cannot separate these things from the effects of our political institutions and it will be extraordinary indeed if they disconnect them from the benign influence of Christianity.

This is the true note—the note which this paper has essayed to demonstrate as proved by time. Mr. Greeley in the case of the Japanese, as in that of monster ships like the *Great Eastern,* was a bad prophet. He argued that the embassy avowed before arrival that it had no ministerial powers except those of signing the treaty and collecting information concerning our currency with a view to better ultimate international adjustment.

But Mr. Greeley saw nothing in this. He referred to the conferences at the mint but was unable to figure out anything feasible. The relations which gold bore to silver in Japan and their artificial value in coinage forbade any basis of equitable exchange. Indeed, he believed, if through their labors at the mint, the Japanese were to adopt the new standards for estimating the values of the precious metals, "it is easy to see that the monetary affairs of the empire might be thrown into great confusion."

But we in this day know that the Japanese were not so simple in their natures, not so sleepy and feeble, as the New York editors supposed. Neither did they go away as ignorant as they came. Nor were they so ignorant when they came. Long before Perry's day Japan had had her martyrs to progress and reform. News from the outside leaked in and shadows of western mechanism and methods fell on the isolated empire. Men like Fujita Toko and Sakuma Shuri had telescopic vision and sensitive hearing. So the envoys of the Tycoon knew that there were advantages to be improved in going to the United States over and above that of signing the Harris treaty. They had the penetration to see that a sound currency and facilities of finance were the pivot of international commercial relations. They were impressed with the fact that international confidence rested chiefly on that scientific accuracy which they saw in the operations of coinage and especially those that guarded the integrity of the standards of fineness. The problem which Mr. Greeley saw as insoluble, was gradually worked out by Ito and Matsukata until Japan possessed a system of coinage modeled on and comparable with that of the United States, and resting on a gold basis.

A letter written to President David Starr Jordan by the distinguished Japanese scientist, Dr. Mitsukuri, in 1900, confirms the trend of this paper as a contention for an unbroken continuity of influence on the development of Japan—in spite of the dismal deliverances of these American prophets of 1860.

Dr. Mitsukuri says:

> The history of the international relations between the United States and Japan is full of episodes which evince an unusually strong and almost romantic friendship existing between the two nations. In the first place, Japan has never forgotten that it was America who first roused her from the lethargy of centuries of secluded life. It was through the earnest representations of America that she concluded the first treaty with a foreign nation in modern times, and opened her country to the outside world. Then, all through the early struggles of Japan to obtain a standing among the civilized nations of the world, America always stood by Japan as an elder brother by a younger sister. It was always America who first recognized the rights of Japan in any of her attempts to retain autonomy within her own territory. A large percentage of foreign teachers working earnestly in schools was Americans, and many a Japanese recalls with gratitude the great efforts his American teachers made on his behalf.

Then, kindness and hospitality shown thousands of youths who went over to America to obtain their education have gone deep into the heart of the nation, and, what is more, many of these students themselves are now holding important positions in the country, and they always look back with affectionate feelings to their stay in America.

In conclusion, it is immensely interesting to see that what Japan came to America for on her first embassy is precisely *that which she has retained as the essential element of her international development.* She afterward went to Germany for army organization and got it; she went to Great Britain for naval ideas and got them; she came here for coinage, exchange, and got them. Moreover, her friendship with the United States has been practically continuous while from 1861 to 1863 she was in hot water with England and France. Incidentally, she carried away our surgery, and no one knows how many minor constructive principles; later she borrowed our banking and postal systems, transplanted our dentistry, and made obeisance to American invention by overspreading the empire with our telegraph.

The embassy of 1860, as was said at the outset, was but the completing touch of the treaties of Perry and Harris. All these constitute a single event but an event that is a gigantic factor in the world's progress. Why the most practical part of it—the embassy—has dropped into such profound oblivion is beyond comprehension. Perhaps it was one of those events which are too broad and too potent to be discerned in less than a half century as the mark of a world-moving era.